Determinants and outcomes of Industrial Policies: Evidence from Italy

by

Letizia Borgomeo

Thesis

Submitted to the University of Warwick

for the degree of

Doctor of Philosophy

Department of Economics

September 2018

Contents

List of Tables

List of Figures

Acknowledgments

These six years have been characterized by a long sequence of failures and very few successes. I often seriously thought I would have not made it and if I made it is thanks to all the people mentioned here. They would probably not benefit from a mention in this thesis but I will benefit from reading this page again in the future and remember how all of this happened.

I want to thank members of the faculty at Warwick for their suggestions and support: Sascha Becker, Abhinay Muthoo, Jeremy Smith and my supervisors Mirko Draca, Fabian Waldinger and James Fenske. A special thanks to James for having agreed to give me the support I needed in a moment in which I felt pretty lost and for giving me an infinite set of extremely useful suggestions. I also acknowledge CAGE for the financial support that partially helped me to gather some of the data used in this thesis. I would also like to thank Rocco Macchiavello and Herakles Polemarchakis for having often reminded me why I initially fell in love with economics.

I would have never survived if it wasn't for some special people that made me feel true Italian vibes from the very first day: Giulia, Rocco, Giulio e famiglia, Monica e Lucio. Grazie rigà, I hope we will always keep in touch. I also want to thank all the other PhD students who made my days much less dull and sometimes even fun: Julia and Ale, my legendary officemates Ghasan, Juanca, and Anik (Anik is added for fairness but he has nothing to do with fun), Federico, David, Mario, Tony and Dimitri and other I do not have on the top of my mind right now. I also want to thank some special women I met when I moved to Bristol and who contributed a lot to help me finding the strength to finish this thesis: Ilaria, Camilla and Myriam. Also thanks to all the people who always supported and spoilt me. A special thanks to Piero, Winni und Beate and Eva.

A huge thank you goes to my friends back home for having always believed in me, also when I did not want to see it. I thank Livia for being always by my side also when I do not deserve it, Chiara for being spontaneous and for having waited for this moment almost as much as I have, Meri for being one of the funniest people I know and my ideal sister-in-law, Isa for her impossible-to -resist smiles (and also thanks to Paulaner and Cri for some unforgettable moments), Marianna for having always trusted me, Puccia for having started a PhD even after all my complaints, Vitto for showing me everyday that it is never too late to change things in life, Turs for being one of the most low-investment and huge-return friendship I ever had the luck to have, Enriquez for all the random beers we had together. A side mention to Glipo, who helped me in one the most awful periods of my life, when my self esteem and strength to go forward were at their lowest level ever. Thanks for being a good friend, a coach and for always making me want to be myself with you.

Thanks to my brothers, because they always make me feel protected. Thanks to each of them for being great examples that no effort should be spared for something you believe in and that a life without purpose is not worth being lived. Thanks to Marco, who made me discover a new form of love, the one for Descarli and Degiobannes. Thank you Eli for sending me videos and pictures that made me laugh and smile on many bad days. Thanks to Lori, because he is my doppelgänger, the best and the worst parts of me; making mistakes together with him is so much better. Thanks to Edo, because he is the best gift I could have ever had. He is in all the places of my life, including this thesis, which he contributed to with proofreading, hugs, brainstormings and motivational speeches. Thanks for sharing with me your rich and unique look at the world. Thanks also to my quasi-sister Guenda, I know you will always be there.

Thank you to mamma e papà, for never leaving me alone and always taking care of me. Thanks papo for making me such a dreamer and thanks mammetta for always being by my side ready to protect me whenever the harsh reality hits in. Thanks for giving me the roots that let me grow.

During the last years I often felt so discouraged that I wondered if it was all worth it. Beyond the academic achievement, the answer to this question is yes because this PhD made me meet two amazing human beings: Martina and Christoph. Thank you Martina, grazie mito, for your real friendship, for which you have opened your heart and your mind so many times even if it was hurting. Thanks for even trying to speak romano to feel closer to me. Thanks for that, thanks for all the times we sang, laughed, drank and cried together. Thanks for being there every day to make sure I am happy. I promise I will always be there for you.

And, finally, thank you Chris. I would need another PhD thesis (no panic, just kidding) just to list all the things I should be grateful to you for. I would have seriously never got here without you. I still cannot find an explanation for all the strength you found to be my lover, my friend, my supervisor and teacher, my coach, my family, my drinking buddy (I guess that was the easy bit), all at once. Thanks for sharing your amazing soul with me, for making me always feel that being myself is the best option, for still being able to surprise me after almost 6 years together. I dedicate this thesis to us and the magic of our love.

Declarations

This thesis is submitted to the University of Warwick in accordance with the requirements of the degree of Doctor of Philosophy. I declare that any material contained in this thesis has not been submitted for a degree to any other university.

Letizia Borgomeo

Abstract

Governments in industrialised countries often resort to direct interventions to alter the structure of the economy and correct market failures. Existing literature in economics has found theoretical and empirical evidence both against and for the case of such policies (Pack and Saggi, 2006). On the other hand, political scientists and economists have often been concerned about the tactical distribution of public funds (Besley and Case, 1995). In this thesis I link these two literatures by using novel datasets and quasi-experimental quantitative methods to understand how public funds may be actually distributed in practice.

In the first two chapters I study the allocation mechanisms of *Cassa del Mezzogiorno* (CasMez), a large placed-based policy targeted at the Italian southern regions from early 1950s to the 1990s. While anecdotal evidence often refers to CasMez as a policy extremely exposed to clientelistic pressures, no quantitative evidence has been provided yet. I reconstruct, to my knowledge for the first time, the allocation of CasMez at the micro level and investigate the mechanisms behind it.

In Chapter 1, I study the political determinants of the allocation of resources in the first years of CasMez activity. By looking at close electoral races in the local elections of 1951-52, I employ a Regression Discontinuity Design (RDD) to test whether alignment of municipalities with the central government had an effect on the distribution of funds at the local level in the years subsequent to the elections. The results show that the central government used CasMez funds to swing marginal municipalities away from the opposition in rural areas which the government considered as at political risk because of their exposure to peasant strikes and land invasions.

In Chapter 2, I study the economic determinants of CasMez over its 40 years of operations. To identify the causal effect of local economic shocks on the allocation of funds, I construct a shift-share instrument and predict local economic growth rate with the weighted average of aggregate industry-level growth rates, wherein weights are derived from the local industry structure in the baseline period. I find that CasMez often responded to local economic shocks by making larger investments in areas that had been growing faster and had better growth prospects.

In Chapter 3, I shift the attention to the impact of one of the most common and least controversial government interventions - support to Research and Development (R&D) investments. By using balance sheet and patenting data, I investigate the effect of an R&D programme targeted at Italian high-tech start-ups. Special features of the programme allow me to consider the allocation of the subsidy as quasi- random and implement an RDD based on cut-off scores assigned by an independent committee to the firms. The results suggest that the subsidy did not increase patent applications and only had a short-run effect on investments in intangible assets.

Chapter 1

Political alignment and development funds: evidence from post-WWII Southern Italy

1.1 Introduction

Central governments transfer large flows of resources to local jurisdictions through intergovernmental grants, to finance sub-national spending and to implement national policies. According to recent estimates, OECD countries annually transfer on average about 5.6% of their GDP (excluding social security transfers) to lower tiers of government (OECD, 2016). Several economists and political scientists have highlighted the role of favoritism (Lizzeri and Persico, 2001; Burgess et al., 2015) and vote-maximisation behaviour of politicians (Wright, 1974; Dixit and Londregan, 1996) in the allocation of these transfers. More specifically, a large body of literature has identified the crucial role of partisan alignment between local constituencies and the central government, finding evidence in favour of it across various countries (Levitt and Snyder Jr, 1995; Veiga and Pinho, 2007; Arulampalam et al., 2009; Brollo and Nannicini, 2012; Bracco et al., 2015). The theoretical foundation for partisan alignmennt builds on the literature pertaining to distributive politics (Cox and McCubbins, 1986; Lindbeck and Weibull, 1987; Dixit and Londregan, 1996) and highlights two crucial elements. Firstly, the central government can use transfers to increase its re-election probabilities by targeting voters with weak partisan attachment (*swing* voters). Secondly, incumbents in local governments can claim a share of the political credit for the transfers. As a result, central governments will transfer more resources to *aligned* local jurisdictions rather than to *unaligned* ones so as to prevent the opposition from getting any political merit.

However, not all transfers generate political credit spillovers. For example, this is the case of *ad-hoc* policies such as place-based policies. Usually not part of the cyclical transfers typical of fiscal decentralisation, these interventions are designed to temporarily respond to particular local inefficiencies or emergency situations following natural disasters. While these policies are very common and many studies have evaluated their outcomes[1], very little is known about the political determinants of their allocation. Even though their design is noticeably discretionary, these policies

[1]See among others, Busso, Gregory, and Kline (2013), Kline and Moretti (2014a) or Becker, Egger, and von Ehrlich (2012).

tend to be *earmarked* and thus committed to economic criteria. In addition to that, ad-hoc policies are often unmistakably government-related and opposition parties might not be able (or willing) to claim any political credit for them. It is then at best unclear whether the results of the literature on distributive politics would also apply to them.

In this paper I study how alignment of municipalities with the central government affected the allocation of funds of a large place-based development policy in post-WWII Italy, *Cassa per il Mezzogiorno* (henceforth, CasMez). CasMez was a development agency created in the second half of 1950 with the aim of implementing a 10-year plan to build key infrastructures (such as land reclamation, aqueducts and roads) in the poorer regions of southern Italy. Contrary to initial intentions, its mandate was renewed multiple times and the activity of CasMez lasted until 1993. Importantly, the central government led by the Christian Democracy party (henceforth, DC), created CasMez to achieve not only developmental objectives but also political aims. In fact, DC considered CasMez as an opportunity to increase its support in the South and to fight the rise of the communist party, with assistance from the US government (Marzotto and Schachter, 1983; Pellè, 2009; Mangullo, 2015). Using a Regression Discontinuity Design (RDD) and novel datasets, I quantify the impact of the results of local elections in 1951-52 on the allocation of the first infrastructural projects funded by CasMez. While my findings do not suggest an overall alignment effect, they do demonstrate the coexistence of alignment and un-alignment effects in different groups of municipalities.

CasMez represents a particularly compelling case study to test distributive politics for a variety of reasons. First of all, this intervention had deep ideological roots. After WWII, the historical gap between the North and the South of Italy had expanded dramatically so that GDP per capita in most of southern regions was only half of the national GDP (Daniele and Malanima, 2007). As a consequence, anti-governmental sentiment was rife in the South especially in rural areas. By the end of the 1940s, land invasions and peasants strikes supported by the Italian communist party (PCI) had dramatically increased (Barucci, 1978). With this development intervention, the central government identified the chance to defend and increase its support in the South. This transpired with the strong backing of the US government and the International Bank for Reconstruction and Development (IBRD), to the extent that 50 percent of CasMez' starting endowment was funded through the European Recovery Program Fund (Pellè, 2009). PCI was in fact, at the time, the largest Communist party in Western Europe and Italy was thus a crucial ally for the US (Urban, 1986). In its oppositional role in the parliament, PCI was openly critical of CasMez for both its association with the US and its Big Push strategy (Pellè, 2009; Lepore, 2014). Crucially then, CasMez was *branded* by the DC government and it plausibly had no political credit spillover favouring the opposition.

A second feature that makes this intervention an interesting case study is its operational structure. CasMez was conceived as an autonomous body with legal personality and technical skills, partially mimicking the experience of the Tennessee Valley Authority (TVA). However, it was not completely independent from political pressures: its members were nominated by the government and the development strategies were designed at the parliamentary or governmental level (Svimez, 2015). Also, the detailed administration and execution of the projects was in the hands of appointed existing agencies, usually provincial or regional departments. Finally, the intervention was conceived of as a multiyear programme but ultimately came to be structured as a series of one-year plans. These characteristics in combination with the temporary nature of CasMez made this intervention discretionary to a substantial extent[2].

The final feature of this case which makes it ideal for testing distributive politics is the simultaneity between the beginning of CasMez activity and the local elections of 1951-52. Importantly, these elections were extremely salient as they happened between the very first[3] parliamentary elections of 1948 and the upcoming

[2]Scholars tend to describe the first years of CasMez activity as the *golden age* of this intervention, especially in terms of independence from the government with respect to the period after 1965, when different law changes allowed for more supervision by the government and the parliament (Cafiero, 2000; Felice and Lepore, 2017; Podbielski, 1978). However, CasMez' substantial discretionary nature was recognized also by CasMez own director (Pescatore, 1961).

[3]The Italian Republic had only become a democracy two years earlier, in 1946, after WWII and 20 years of fascist dictatorship. Young democracies are also typically thought as being more prone

ones of 1953. In particular, DC, after the strong victory obtained in the 1948 elections, with 48% of overall votes, was concerned about the impact that increasing support for communist and monarchic parties would have on the 1953 elections (Possanzini, 2000). As a matter of fact, DC lost an overall 13 percentage points of support in the 1951-52 local elections and its political strategy was so affected that the government pushed for the adoption of a new electoral law that would guarantee them a majority bonus in the upcoming parliamentary elections[4]. DC undoubtedly perceived the results of 1951-52 local elections as a crucial *Litmus* test according to which it could redesign its political strategy (Corbetta and Piretti, 2009).

To analyse the political determinants of the allocation of CasMez' funds, I construct a novel municipality-level data of the projects funded by CasMez in its first years of activity. The empirical identification of the causal effect of electoral results on allocation of transfers is demanding. This is because several socio-economic factors such as pre-existing corruption levels or income can influence both dimensions. I employ an RDD and compare the allocation of funds in municipalities where DC won by a small margin to municipalities where DC lost by a small margin. The baseline results indicate no statistically significant evidence for an alignment effect on the money allocated by CasMez to the municipalities where DC had just won within 5 years subsequent to the elections. Additionally, I find no effect on the *quantity* and the *average size* of projects funded by CasMez. The results are robust across a wide range of parametric and non-parametric specifications. To explore the potential mechanisms behind these results, I first look at close electoral races for PCI to test for the presence of un-alignment effect. I find that unaligned municipalities, where PCI had just won were 20% more likely to be targeted by CasMez than municipalities where PCI had just lost.

In the light of these findings, I re-examine the baseline results by splitting municipalities into agricultural areas which were particularly perceived to be at risk of Communist land occupations and non-agricultural areas. This analysis reveals

to corruption and clientelism, see for instance Keefer (2007) or Veiga and Pinho (2007).

[4]The *ad-hoc* nature of this law was harshly criticised, to the point that it was called the *scam law*.

that the baseline null-results were masking an important heterogeneity, namely the coexistence of alignment and un-alignment effects in different subsamples. In particular, I find that while *urban* municipalities received more money if aligned, the opposite was true for *rural* municipalities. Additional analysis also shows that the effect is mainly identifiable on the extensive margin. A potential explanation of these results is that rural areas were considered at greater risk by DC because of their exposure to peasant strikes and land invasions. Thus, given the absence of political credit spillovers, DC might have used CasMez funds to swing marginal municipalities away from the opposition. For urban areas, instead, the typical partisan alignment model predictions would apply and DC might have used the funds to reward its supporters, to build party strongholds or to support the rent-seeking behaviour of the notables. Overall, I conclude that research on distributive politics might benefit from considering the role that absence of political credit spillovers and other important political features of the constituencies such as political instability can play.

This paper builds on the seminal literature on distributive politics (Cox and McCubbins, 1986; Lindbeck and Weibull, 1987; Dixit and Londregan, 1996)[5]. In particular, it contributes to the expanding literature on the effects of political alignment (see, amongst others: Levitt and Snyder Jr, 1995; Larcinese, Rizzo, and Testa, 2006; Veiga and Pinho, 2007; Arulampalam et al., 2009; Brollo and Nannicini, 2012; Johansson, 2003; Albouy, 2013; Bracco et al., 2015; Muraközy and Telegdy, 2016; Baskaran and Hessami, 2017).

Three works are very close to the current study as they provide the crucial theoretical framework that motivates this paper. Arulampalam et al. (2009), Brollo and Nannicini (2012) and Bracco et al. (2015) all present theoretical models that rely crucially on the presence of political credit spillovers of central transfers that

[5]Briefly, scholars have put forward two contrasting views based on different hypotheses about which voters are more likely to be responsive to distributive benefits and thus more likely to be targeted by governmental transfers. The classic machine politics argument, as in Cox and McCubbins (1986), is that risk-averse politicians should prefer to target their core base of supporters. The alternative model, the swing-voters model, (Lindbeck and Weibull, 1987; Dixit and Londregan, 1996) predicts that parties target election districts with a largest share of ideologically indifferent voters, who can change the electoral results at the margin.

can be shared by the local unaligned incumbent. The model proposed by Brollo and Nannicini (2012) predicts that there is no tactical distribution in the absence of political credit spillovers. Arulampalam et al. (2009) and Bracco et al. (2015) obtain alignment effects in their models when the increased voter utility or *goodwill* of the transfers goes to the non-aligned incumbent. Particularly in Arulampalam et al. (2009), the share of goodwill going to the incumbent must be large enough (greater than a half) and is taken as exogenous for the alignment effect to arise. On the other hand, Bracco et al. (2015) endogenise this share and obtain that the incumbent takes full credit for the transfers (i.e., the share is equal to 1). I complement the results of these works by testing the alignment effect in the context of an *ad-hoc* policy, in which the share of goodwill that the unaligned incumbent can take credit for is plausibly equal to zero.

To the best of my knowledge, this is the first paper to look at the effect of alignment on transfers from whence political credit spillovers can be ruled out. A few papers investigated the political determinants of place-based policies. Muraközy and Telegdy (2016) and Dellmuth and Stoffel (2012) looked at the EU structural funds and found positive alignment effects in Hungary and Germany respectively. However, the context of the EU funds is very different from the one proposed in this paper as local authorities can initiate the application for funds. This means that local politicians can influence the allocation process by determining the *demand* for funds. Moreover, the budget and eligibility criteria of EU funds are defined at the supranational level by the European Parliament and the Council of the European Union.

Overall, most of the recent empirical literature on political alignment has identified a positive alignment effect of fiscal transfers in several countries. Based on the literature surveyed, only two papers have not found evidence for an alignment effect. Ward and John (1999) report that in one specific year in England the central government transferred more grants to marginal local authorities held by the party of the opposition. Owing to advances in empirical methods, this current study provides a more robust identification through RDD. Baskaran and Hessami (2017) also used an RDD and found that budget support transfers are used by a German state in very heterogeneous ways and observed an alignment effect only in periods when the state government has more local support. Their results parallel mine as they show that the effect of alignment varies according to local political conditions. However, the role of political credit spillover is unclear in their analysis.

Finally, this paper contributes to the literature on post-WWII Italy and CasMez. In particular, Marzotto and Schachter (1983) looked at CasMez investments over its first 25 years of activity and investigated their relationship with local electoral behaviour. Using a random sample of municipalities, they provided qualitative evidence that DC-core electoral districts were least likely to receive economic rewards. My work builds on theirs by providing quantitative evidence[6] and focusing on *swing* municipalities. A growing body of research has recently looked at CasMez, especially from a historical point of view (Lepore, 2014; Felice and Lepore, 2017; d'Adda and de Blasio, 2017). I contribute to this literature by providing the first quantitative test of the political determinants of CasMez allocations.

The rest of the paper is organised as follows. The next section describes the political background against which CasMez functioned and the elections were held. Section 1.3 describes the dataset and the main variables of interest. Section 1.4 describes the identification strategy and provides some evidence for its validity in this setup. Section 1.5 shows the main results. Section 1.6 presents additional results and discusses the mechanisms and the final section furnishes the conclusion to the paper.

1.2 Institutional background

This section provides more details about the origins of CasMez, the role it played in the propaganda of DC and the political salience of 1951-52 local elections. After

[6]To my knowledge, the only other paper that empirically looks at distributive politics in postwar Italy is Golden and Picci (2008). However, they do not look at the alignment effect. In fact, they study how the results of the parliamentary elections affect infrastructural transfers (unrelated to CasMez) from the central government to provinces. Through a panel estimation, they show that individually powerful deputies tend to target their core constituents.

WWII, and 20 years of fascist dictatorship, Italy became a Republic in 1946 following a national referendum wherein 54% of the voters picked the Republic against the Monarchy as the system of government. The initial years of the Republic were deeply influenced by the cold-war confrontation. The first parliamentary elections of 1948 showed an extremely polarised country: the Christian Democratic party (DC) and Communist-Socialist coalition (FDP)[7] alone obtained almost 80% of the votes. The electoral campaign[8] was almost totally focused on the blocks-confrontation and led DC to win with 48% of the votes (Corbetta and Piretti, 2009).

In the meantime, the gap between the North and the South of the country dramatically widened[9]. Structural economic differences had been made worse by the damages wrought by the war (Pellè, 2009). Also, the southern regions benefitted very little from the funds of the European Recovery Program (Fauri, 2010). The DC government started considering this issue especially due to the emergent social tensions in the agricultural sector. These were seen as a threat to the political stability of the country and to its integration in the Western bloc (Crafts and Magnani, 2013).

It is against this background that DC which was strongly supported by the IBRD decided to actively encourage the development of the southern regions with an *intervento strardinario*, i.e. special intervention. The main instrument of this policy was the creation of an independent funding agency which was modelled after the experience of the Tennessee Valley Authority (TVA) in the US and designated to implement highly technical infrastructural projects to push market forces to generate the development of the South[10].

Understanding the political motives behind the creation of CasMez is ex-

[7]This coalition was created for these elections, after which Communists and Socialists started running as two different parties,PCI and PSI.

[8]See figure 1.31 in Appendix 1.C.3.

[9]Scholars of different disciplines have been studying the North-South divide, *the southern question*, extensively and are far away from having reached a common view. However, most of them agree that the institutional and economic differences before unification in 1861 have played a crucial role (for a recent discussion on this see Federico and Vasta, 2017).

[10]Even if a causal relationship has not been established yet, scholars tend to agree that CasMez contributed to the unique phenomenon of convergence between southern and northern regions between 1951 and 1971 (Felice, 2015; Podbielski, 1978; Crafts and Magnani, 2013).

tremely pertinent to interpreting the results of this paper. An internal IBRD report(WB, 1951)[11] leaves no ambiguity about the existence of these motives:

> While the development of the south is an old issue in Italy, [...] the current development program [..] was authorized in an effort to counter the plans proposed by the communists and to increase, if possible, the political prestige of the Government in these areas.

The intervention was also included in the propaganda of DC, especially against the communist party (PCI), as figures 1.1 and 1.32 show.

Most importantly, the opposition parties identified in this intervention, at least in the very first years, the attempt of DC to increase its support and the intention of the US government to retain Italy on its side. Two extracts from parliamentary speeches[12] of two MPs, Giorgio Amendola and Mario Alicata, from PCI are self-explanatory with regard to this issue:

> [...] We can very well foresee what this CasMez will do to all economic, social and political sectors in the Italian south. It is a powerful tool of electoral and political corruption that you want to put up, for your own party's interests, to establish the empire of your regime in the Italian south.

> [..] It is an instrument to penetrate the American capital and influence into the South. [..]. this American inspiration behind CasMez is a threat for the South.

In particular, most of the political tension around this intervention was related to the rural areas. As a matter of fact, the majority of funds were initially related to large investments for roads, aqueducts and land reclamation and thus targeted at agricultural areas which constituted a large share of the southern regions[13].

[11]See Appendix 1.C.1 for full text.
[12]See Appendix 1.C.2 for full texts.
[13]At the end of the 1940s, 34% of GDP in the South was coming from agriculture, against 19% in the Centre-North (Podbielski, 1978).

Figure 1.1: **DC's propaganda**. The poster says : "Facts speak: 1280 billions for Mezzogiorno", "Reconstruction in Italy has a name: Democrazia Cristiana" and "Vote"

Bernardi (2006) highlights that one of the reasons why the actual areas of intervention were not established when CasMez was created was to prevent communists from encouraging peasants to occupy target lands.

In this context, the local elections of 1951-52 were very important for DC so as to test its level of support, especially in the South, after the big victory of 1946. These elections were supposed to take place in 1950 and were even postponed[14] to allow the parliament to modify the electoral system first. The original electoral system, used in the 1946 local elections, was proportional for municipalities with less than 30 thousands inhabitants and majoritarian for the larger ones[15]. The change was mainly based on the extension of a majoritarian rule to smaller municipalities. DC was trying to test the transition from a proportional to majoritarian system that would subsequently be proposed for adoption in the 1953 parliamentary elections with the intention being to maintain a strong control of the parliament thanks to a majority bonus.

1.3 Data

This analysis is based on three main data sources: the CasMez project-level dataset, the results of the 1951-52 local elections and the Industry and Population censuses from 1951. The data for CasMez has been provided by the Ministry for Economic Development and includes information on timing, location, size and types of the universe of projects funded by CasMez. To merge this information with the census data, I aggregate the projects to the municipality level and reconstruct the borders of municipalities as of 1951. While almost 90% of the projects are matched to a single municipality in the Ministry's database, 48% of the total funds are coded as being targeted to public works projects referred to as pluricomunali, i.e. multi-municipality. For those *multi-municipality* projects for which a list of targeted

[14] Law 255/1950.

[15] More specifically: municipalities with less than 30 thousand inhabitants had a proportional system with D'Hondt method for the allocation of seats. This system was also applied to province-capital municipalities, i.e. the main municipality within a province, where p.a. offices at province level are usually located. Municipalities with more than 30 thousands inhabitants had instead a majoritarian system with block voting.

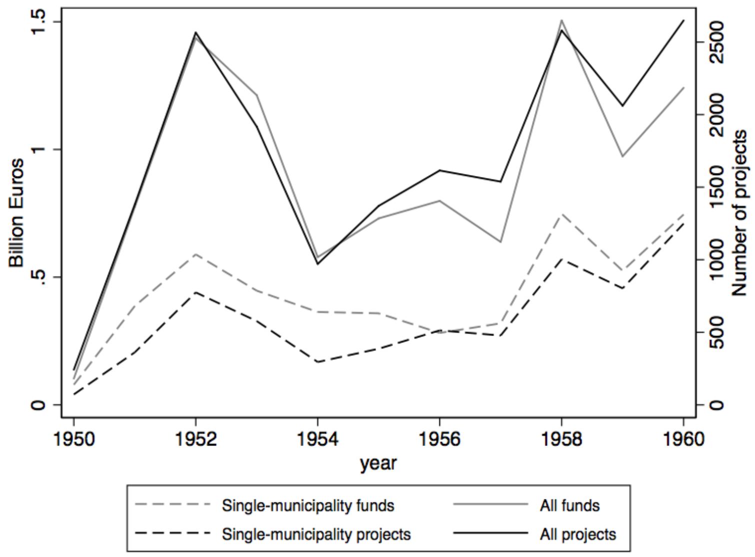


Figure 1.2: **CasMez money and number of projects in the first decade for public works, by target**. The patterns of the lines depend on the target of the projects: dashed lines measure single-municipality projects, while solid lines measure multi-municipality projects. Colours depend on the metric used. Black is for the money invested, measured on the left axis and grey is for the number of projects, measured on the right axis. Funds in each year are measured as any funds allocated during that calendar year.

locations is not available, I have extracted location names and other geographical references from other fields of the project entry[16]. For each municipality, I can then reconstruct the number of projects and the cumulated money received up to 5 years after the elections had taken place. All monetary values are converted to their 2011 value, using reconversion coefficients provided by the Italian Statistical Office (ISTAT).

Figure 1.2 shows how CasMez intervention evolved during the first years of its activity. In particular, the chart shows that: i) most of the projects in this period are *multi-municipality* projects and ii) the trends in amount of money and number of projects are very similar[17].

The Population census provides the count of residents and other demograph-

[16]More details on how I allocate these projects and on the data cleaning process in general can be found in the Appendix to Chapter 2 of this thesis.

[17]See also Appendix 1.A for figures 1.12 and 1.13. The former shows the trend in CasMez money over years, comparing the measure with and without *multi-municipality* projects. The latter zooms into the period studied in this paper, showing how *multi-municipality* projects consisted of a large share of overall projects.

ics, such as literacy level, household conditions and, most importantly, residents active in agriculture. The Industry census provides the number of employees in each municipality, excluding the following sectors: agriculture and fishing, education, health services and public administration.

The results from the administrative elections have been collected from statistical publications[18] of the Elections Department of the Ministry of Interior and then digitised. As the electoral system was slightly different for different population thresholds, this paper only focuses on municipalities with less than 10 thousands inhabitants as they make up to 90% of the Italian municipalities in the period under study. The electoral system for municipalities with less than 10 thousands inhabitants and maximum 20 seats in the local council[19] was majoritarian with limited vote[20]. In this system, each voter can cast a vote for candidates (but not the list they belong to) but the maximum amount of votes she can cast is equal to 4/5 of the total number of seats[21]. The seats of the local council are allocated to the candidates with the highest number of votes. As the system is parliamentary, the mayor is elected by the council members after the elections.

For each list in a given municipality, the publication reports[22] the *estimated* number of votes received and seats won. The votes are estimated because electors cast votes for *candidates* and not for lists. In order to approximate and measure support to lists or parties the publication reports the arithmetic mean of the preferences given to all candidates in that list. This is computed as the ratio between the total number of votes the list received and the number of candidates in the list. However, this metric is problematic for measuring the strength of different candidates within

[18]For municipalities above 10 thousands inhabitants: Ministero dell'Interno - Divisione Servizi Elettorali - Direzione Generale dell'Amministrazione Civile (1954a) and Ministero dell'Interno - Divisione Servizi Elettorali - Direzione Generale dell'Amministrazione Civile (1954b). For municipalities below 10 thousands inhabitants: Ministero dell'Interno - Divisione Servizi Elettorali - Direzione Generale dell'Amministrazione Civile (1954c).

[19]Sicily had its own electoral system and is for this reason excluded from the analysis.

[20]Larger municipalities had a majority premium with the possibility of coalitions. The voter would cast the vote to the list or coalition and the list with relative majority would get then 2/3 of the seats.

[21]The rationale of this system is to have minorities represented by preventing the majoritarian party from having all the council seats.

[22]See figure 3.2 in Appendix 1.A.1

Table 1.1: Winning Parties and votes' distribution

	Seats' Majority								
Votes' Majority	PCI-PSI	C-Left	Centre	DC	DC_Others	Right	Others	Ind	Total
PCI-PSI	390	0	0	2	3	1	0	0	396
C-Left	0	20	0	1	0	0	0	0	21
Centre	0	0	70	1	0	0	0	0	71
DC	4	0	2	649	1	3	1	0	660
DC_Others	6	0	0	1	593	0	2	1	603
Right	1	0	0	3	1	148	0	0	153
Others	3	1	0	1	1	0	148	0	154
Ind	4	0	0	9	6	0	0	174	193
Total	408	21	72	667	605	152	151	175	2,251

Notes: The table reports the counts of municipalities where each party won the majority of seats or votes. The sample includes all digitized elections for municipalities targeted by CasMez and thus differ from the sample of the analysis. Parties are as follows: PCI-PSI is the Communists (PCI) and Socialists (PSI) coalition, C-Left includes the more centre-wing of the socialist party (PSLI), and several social-democratic parties (PSDI, PSULI, PSU) and often the republican party (PRI). Centre includes all Centre's parties, excluding DC, that is mainly the liberals (PLI).

the same list, especially as coalitions including candidates with different political views are quite common in small municipalities. Most importantly, two particular cases can arise: i) coalition A can obtain more votes than coalition B but coalition B ends up with more seats because more candidates have lots of preferences and ii) estimated votes can end up being greater or equal than the number of voters if there are incomplete lists or if the allocation of seats occurs considering also subdivisions of municipalities. However, such cases rarely transpire and so do not constitute a concern. Table 1.1 shows that case i) does not happen very frequently and case ii) only happens in 5% of all municipalities in southern regions.

Excluding Sicily which had its own electoral system, four invalid elections, local elections occurring in 1953 or later[23] there are overall 1736 municipalities across 8 southern regions[24]. As the identification strategy relies on the comparison between municipalities where DC just won and municipalities where it just lost, I restrict the sample to electoral races in which one of the first two lists was aligned with the central government, i.e. DC. This restricts the sample to 1522 municipalities. However, in local elections, DC would often run with coalitions, usually with liber-

[23]Only 10% of the elections occurred in 1953 or later.

[24]I also exclude municipalities that are in regions that are not part of the South and were targeted by CasMez only to a minimal extent: Emilia-Romagna, Marche, Toscana and Umbria. The results are anyways robust to their inclusion in the dataset.

als and republicans with distinguishably different political views. To look at *pure* political alignment then I restrict the sample to municipalities where DC is running alone. The final sample with municipalities where DC is running alone consists of 794 municipalities. Tables 1.2, 1.3 and 1.4 show basic summary statistics in these three samples. On average, these samples do not differ significantly.

The main outcome variable is the inverse hyperbolic sine transformation (henceforth, Arcsinh) of funds targeted to municipalities. I use the Arcsinh transformation, as suggested by Burbidge, Magee, and Robb (1988), to reduce the influence of extreme values but especially also retain all zero values[25] I also construct additional measures of the intervention. First, to estimate the effect on the *extensive margin* I construct an indicator variable equal to one when the municipality was targeted at least once. Secondly, to test the alignment effect on the *quantity* of projects I construct the number of projects every 1000 inhabitants, using the resident population in 1951. Finally, I measure the average size of the projects in a municipality by dividing the total funds by the number of projects. I then measure these variables in different years after the elections. As the standard term for the municipality council and the mayor was four years, I look at results for up to 5 years after the election to investigate whether there is suggestible evidence about the effect of the 1956 local elections.

1.4 Identification Strategy

1.4.1 Fuzzy RDD

The main challenge to empirically identify the causal effect of alignment on the allocation of funds is selection bias. The distribution of political preferences is determined by several factors and thus municipalities aligned with the central government are noticeably different from unaligned ones. The effect of alignment cannot be disentangled by the effect of other cofounders through a mere comparison between the money allocated in aligned and unaligned municipalities. This paper

[25]Except for very small values, the inverse sine is approximately equal to *log2y*. Results are robust to the use of *log(1+y)*.

Table 1.2: Summary statistics (All races)

Variable	Mean	Std. Dev.	Min.	Max.	N
Dummy if targeted by CasMez up 4 years after elections	0.63	0.48	0	1	1736
Log of money allocated up to 4 years after elections	8.72	6.73	0	18.66	1736
Number of projects every 1000 inhabitants	0.78	1.14	0	12.01	1736
Average project size, thousand euros (PPP)	308.62	1080.3	0	31706.99	1736
Industrial jobs per capita	0.07	0.04	0.02	0.53	1736
Illiteracy rate	0.28	0.09	0.03	0.70	1736
Log of population	7.72	0.65	5.46	10.18	1736
Plants per capita	0.04	0.01	0.01	0.14	1736
% of population active in agriculture	0.39	0.14	0.03	0.84	1736
% of households with kitchens in 1951	0.95	0.08	0.36	1	1736
% of households with electricity in 1951	0.63	0.24	0	0.99	1736
% of households with no water/electricity	0.01	0.02	0	0.32	1736
Turnout at elections	86.92	6.54	52.7	98	1736
Votes' share of DC (also if in coalition)	0.47	0.23	0	1	1736
Votes' share of PCI	0.2	0.21	0	0.93	1736
Votes' share of right wing coalitions	0.09	0.17	0	1	1736

Table 1.3: Summary statistics (DC races: coalitions)

Variable	Mean	Std. Dev.	Min.	Max.	N
Dummy if targeted by CasMez up 4 years after elections	0.64	0.48	0	1	1521
Log of money allocated up to 4 years after elections	8.75	6.73	0	18.66	1521
Number of projects every 1000 inhabitants	0.79	1.15	0	12.01	1521
Average project size, thousand euros (PPP)	315	1135.31	0	31706.99	1521
Industrial jobs per capita	0.07	0.04	0.02	0.53	1521
Illiteracy rate	0.28	0.09	0.03	0.70	1521
Log of population	7.71	0.65	5.46	9.15	1521
Plants per capita	0.04	0.01	0.01	0.11	1521
% of population active in agriculture	0.4	0.14	0.03	0.84	1521
% of households with kitchens in 1951	0.95	0.08	0.36	1	1521
% of households with electricity in 1951	0.62	0.24	0	0.99	1521
% of households with no water/electricity	0.01	0.03	0	0.32	1521
Turnout at elections	86.95	6.53	52.7	98	1521
Votes' share of DC (also if in coalition)	0.53	0.17	0.09	1	1521
Votes' share of PCI	0.2	0.21	0	0.72	1521
Votes' share of right wing coalitions	0.09	0.16	0	0.84	1521

Table 1.4: Summary statistics (DC races: no coalition)

Variable	Mean	Std. Dev.	Min.	Max.	N
Dummy if targeted by CasMez up 4 years after elections	0.62	0.49	0	1	794
Log of money allocated up to 4 years after elections	8.44	6.75	0	17.75	794
Number of projects every 1000 inhabitants	0.78	1.16	0	10.4	794
Average project size, thousand euros (PPP)	0.26	0.58	0	7.58	794
Industrial jobs per capita	0.07	0.04	0.02	0.49	794
Illiteracy rate	0.28	0.1	0.03	0.70	794
Log of population	7.63	0.66	5.64	9.15	794
Plants per capita	0.04	0.01	0.01	0.11	794
% of population active in agriculture	0.4	0.14	0.03	0.84	794
% of households with kitchens in 1951	0.95	0.08	0.36	1	794
% of households with electricity in 1951	0.61	0.25	0	0.99	794
% of households with no water/electricity	0.01	0.02	0	0.19	794
Turnout at elections	87.26	6.41	59.3	98	794
Votes' share of DC (no coalition)	0.52	0.16	0.09	1	794
Votes' share of PCI	0.17	0.19	0	0.72	794
Votes' share of right wing coalitions	0.1	0.17	0	0.84	794

provides a quasi-experimental setting to isolate this effect by looking at close elections. Analysing the causes or consequences of policy by comparing constituencies where a party just lost to constituencies where it just won is a well established and standard tool in political economy (Lee, Moretti, and Butler, 2004; Ferreira and Gyourko, 2009; Pettersson-Lidbom, 2008). The power of this methodology lies in the fact that, under some testable conditions, the treatment status can be thought as being quasi-random around the cut-off so that the outcome for constituencies below the cut-off is a proper counterfactual for the outcome of constituencies above.

Define $F_i(1)$ as the total funds received by municipality i after the elections if DC obtains the majority of seats in the local council and $F_i(0)$ as the potential funds received by the same municipality if another party wins the majority of seats. The causal effect of alignment is defined as $E[F_i(1) - F_i(0)]$. Now define the votes' margin of victory for DC (VMV) as the difference in the share of votes obtained by DC and the share of votes obtained by the runner-up when DC wins and the opposite when DC loses. I am interested in estimating β in the following model:

$$Y_i = f(VMV_i) + \beta Alignment_i + \epsilon_i \tag{1.1}$$

where VMV is the *running variable* that is related to the outcome through the generic functional form $f(.)$. As described in section 2.3, majority of votes does not always imply majority of seats. Thus, the alignment of the municipalities with the central government is not a deterministic function of VMV and the treatment assignment of DC having the majority of votes and the treatment status of DC having the majority of seats do not coincide. I define the treatment assignment dummy Z_i such that $Z_i = 1(VMV_i \geq 0)$, so that municipalities are assigned to the treatment when VMV is above the cut-off of 0. If Z is as good as randomly assigned around the cut-off, any differences in the outcome within a narrow bandwidth can be interpreted as the causal effect of the *assignment to the treatment*, $\widetilde{\gamma}$ in the following model:

$$Y_i = f(VMV_i) + \widetilde{\gamma}1(VMV \geq 0) + \widetilde{\xi}_i \tag{1.2}$$

As in an IV framework, the effect of the *treatment* can be obtained by rescaling the intention-to-treat (ITT) coefficient $\widetilde{\gamma}$ by the change at the cut-off in the probability of treatment. In other words, the assignment to the treatment can be used as an instrument for the treatment itself and the coefficient of interest β is the ratio between the reduced form and the first stage coefficient, γ from equation (1.3).

$$Alignment_i = g(VMV_i) + \gamma 1(VMV \geq 0) + \xi_i \tag{1.3}$$

$$\beta = \frac{\widetilde{\gamma}}{\gamma} \tag{1.4}$$

For the ratio in (1.4) to be valid, the monotonicity assumption must be satisfied. In this context, this is very plausible as it only implies that DC having the majority of votes does not *decrease* the probability of alignment. Then, the ratio in (1.4) is the local average treatment effect (LATE) on the compliers around the cut-off.

Before estimating β, I create a set of RD plots to investigate the presence of a discontinuity in the outcome variable around cut-off. This analysis is important as it sheds light on the functional form linking the running variable to the outcome. I then estimate both parametric and non-parametric regressions, in which I use Z_i as an instrument for the alignment dummy, $Alignment_i$. First, I allow different polynomial orders P of the running variable on each side of the cut-off and estimate the following regression on the whole sample:

$$Funds_i = \sum_{k=0}^{P} \delta_k VMV_i^k + \sum_{k=0}^{P} \gamma_k (\widehat{Alignment}_i * VMV_i^k) + \widetilde{\xi}_i \tag{1.5}$$

$\widehat{Alignment}_i$ is the predicted value from a first stage regression, as in equation (1.3), and γ_0 measures the alignment effect for the observations at the cut-off. Secondly, I restrict the sample to observations around the cut-off only and estimate a local polynomial regression within different bandwidths and inference methods, as suggested in the literature (Imbens and Lemieux, 2008; Lee and Lemieux, 2010; Calonico, Cattaneo, and Titiunik, 2014).

1.4.2 Testing assumptions' validity

The estimation of β as described in section 1.4.1 is valid only if the treatment is as good as randomly assigned around the cut-off. To check the validity of this assumption, I investigate whether there is evidence for manipulation of the running variable and for relevant differences in municipality characteristics around the cut-off.

First, I look at the density distribution of VMV to see if there is any bunching below or above the cut-off. Empirical literature has highlighted how close elections can often be prone to high degrees of manipulation as incumbents are very different from other candidates especially at the cut-off (Grimmer et al., 2011; Caughey and Sekhon, 2011)[26]. However, in this paper, as in Brollo and Nannicini (2012), the running variable is the margin of victory of the aligned party and thus differences in terms of probabilities of winning should not affect the validity of the identification strategy. Also, the limited vote structure of the majoritarian system used in these local elections implies that it is less clear to politicians which elections will be close because the way to obtain the majority of seats is to have as many as possible votes per person as possible rather than the largest possible number of votes. This prior is confirmed by figure 1.3 that shows that the density is smooth around the cut-off and

[26]See however Eggers et al. (2015) for a *defence* of RD's validity in close electoral races.

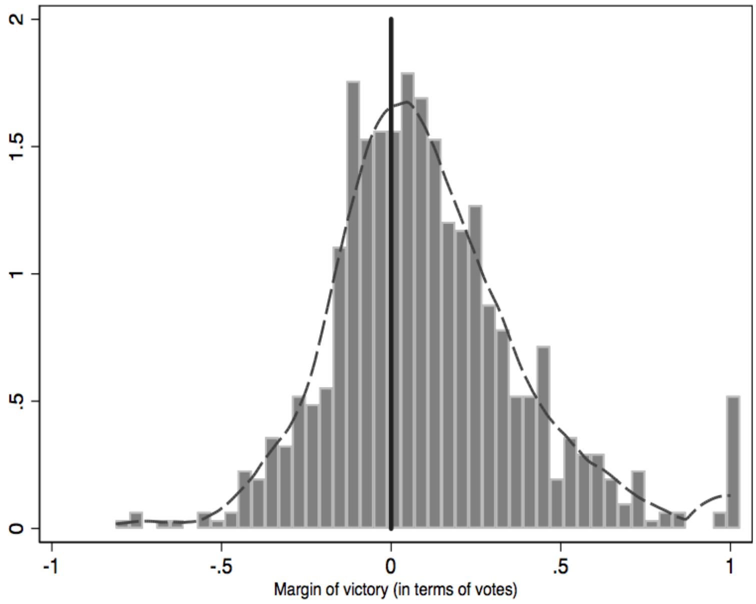


Figure 1.3: **Density distribution of the running variable** This graph shows the density distribution of the running variable together with the plot of kernel density estimates.

by density tests proposed in the literature. The McCrary's test (McCrary, 2008) yields a t-ratio of 0.8 and the test proposed by Cattaneo, Jansson, and Ma (2017) yields a t-ratio of 0.11. Both tests cannot reject the null of the difference in densities at the cut-off being equal to 0.

If the treatment is as close as quasi-random assigned, municipality characteristics should be balanced around the cut-off. To test this, I employ the empirical methodology described in section 1.4.1 to conduct some falsification tests on core observables. In particular, I look at socio and economic variables that are likely to be correlated with both the allocation of funds and the electoral results, such as population size, size of the agricultural sector (measured as the share of population active in agriculture), illiteracy rate, development in terms of household infrastructures, number of industrial jobs and plants per capita.

First, I plot these variables against the running variable, VMV. Any jump at the cut-off would signal the presence of statistically significant differences between municipalities where DC just lost and municipalities where it just won. Figure 1.4

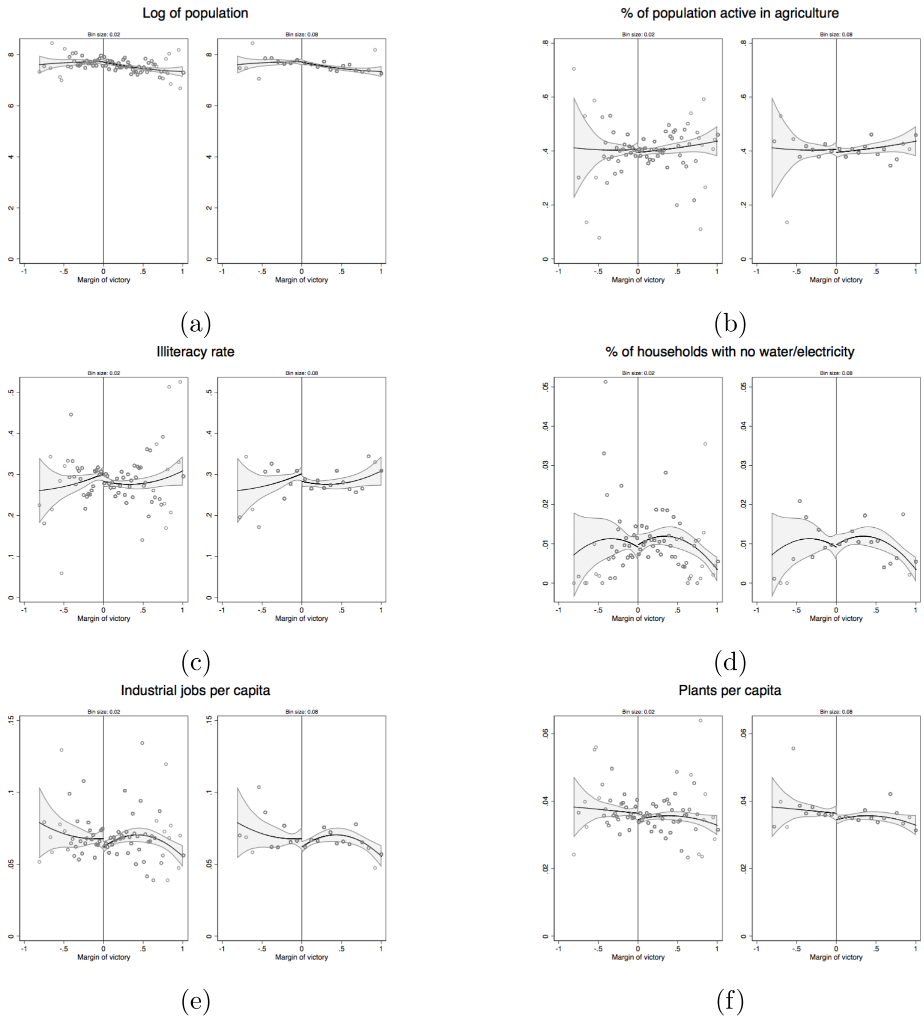


Figure 1.4: **Covariates and margin of victory.** These graphs show the average of each variable within bins of the difference in the share of votes between DC and the main opponent party. All metrics are based on population and industry census of 1951. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

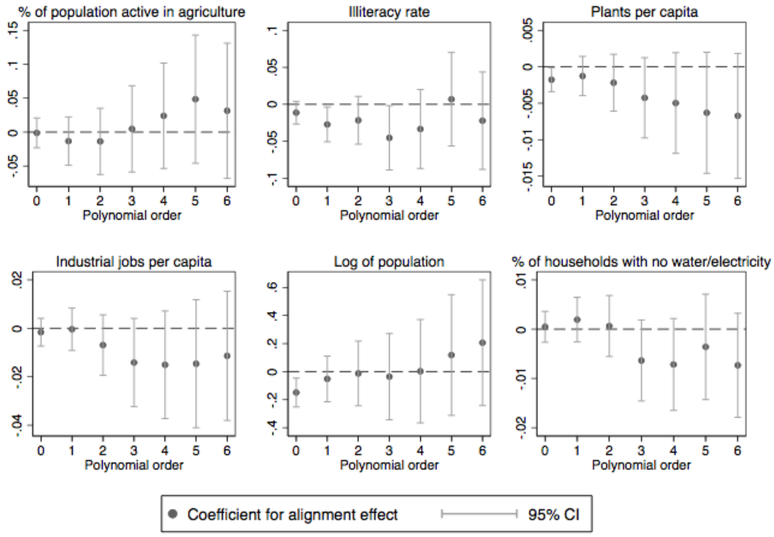


Figure 1.5: **Placebo regressions.** These graphs show the coefficients and the confidence intervals from the estimation of γ_k in equation (1.5) for each covariate by different values of P.

plots the mean of each variable within two different sizes of bins of the running variable[27] and fits a quadratic polynomials on each side of the cut-off. All the variables have a smooth pattern around the threshold. Secondly, I estimate the regressions described in section 1.4.1 to check whether any statistically significant jump can be detected at the cut-off. Figure 1.5 shows the coefficients estimated for different polynomial orders of equation (1.5). None of the variables is significantly correlated with the alignment dummy.

1.5 The effect of alignment of funds' allocation

This section consists of four subsections. The first one presents the RDD graphical analysis and the second one shows the estimation results for the main outcome. The third section looks at alternative outcomes while the fourth one presents further robustness checks.

[27]Results are similar for other bin sizes.

1.5.1 Graphical Analysis

Figure 1.6 plots the funds allocated to municipalities after the elections against the running variable, the votes' margin of victory (VMV), as defined in section 1.4.1. Each graph shows the funds - measured as the inverse hyperbolic sine of the cumulated funds - received up to x years from the year of election. The circles represent the mean of the outcome variable within bins of 0.1 width of the running variable to the left and right of the cut-off; the fitted lines and shaded areas represent the fitted values and confidence intervals based on a quadratic polynomial regression on each side of the cut-off. The graphs do not show any differences in the outcomes between municipalities where DC just lost or just won and, even if the confidence intervals are narrow and would lead to over-rejection.

These RD plots are very similar for different specifications of the underlying regression, of bin size and of the outcome variable. Appendix 1.B.1 reports the same plots with a linear or cubic fit (figures 1.17 and 1.18), with different bin sizes (figures 1.15 and 1.16) and with money measured as logarithm or in per capita levels (figures 1.19 and 1.20).

This null result might hide an alignment effect on two other dimensions. First, projects could be targeted to aligned municipalities only (*extensive margin*). To investigate this, figure 1.21 in Appendix 1.B.1 reproduces the same RD plots as in figure 1.6 for a dummy variable equal to 1 if the municipality received at least one grant. Again, there is no significant jump at the cut-off[28] so aligned municipalities are not more likely to be targeted by CasMez than unaligned municipalities.

Even if there is no effect of alignment on the *extensive* margin of CasMez allocation, there could still be one on the *intensive* margin. Projects could be targeted to both aligned and unaligned municipalities, but disproportionally more to one of the two groups. To test this, I restrict the sample to municipalities that received at least one grant in the first year after the election. Selecting the sample on the dependent variable can lead to selection bias in estimations and this test needs to

[28]Results are also robust to different polynomial orders and bin sizes. The charts are available upon request.

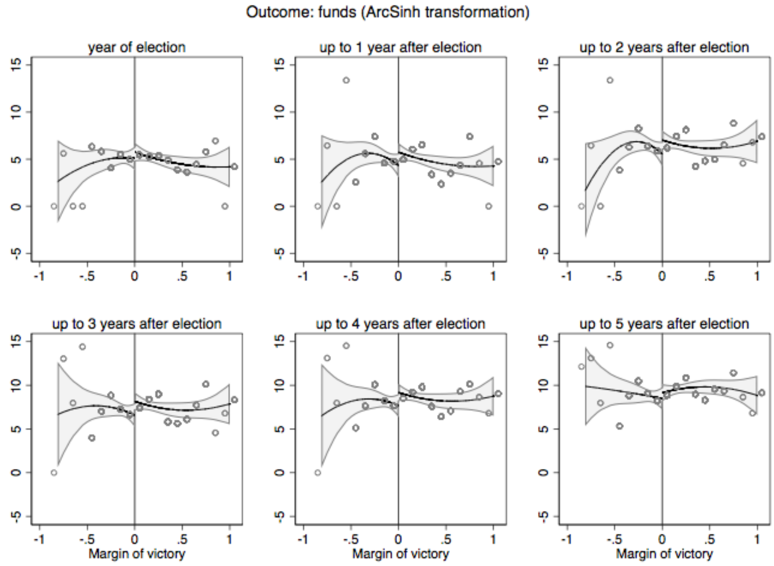


Figure 1.6: **Inverse hyperbolic sine transformation of funds allocated against (VMV) by years after the election.** These graphs show the average money allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

be interpreted with this caveat in mind. Overall, this condition removes more than half of the observations from the sample, thereby leaving us with 303 municipalities. Figures 1.22 in Appendix 1.B.1 shows the RD plot on this subsample of municipalities. The results indicate that also among recipients, there is no significant difference in the grants received between areas where DC just won or just lost. Overall, RD plots show that the alignment of municipalities with the central government does not significantly affect the allocation of funds on either the extensive or the intensive margin. The next section shows the results from the regressions described in section 1.4.1 which offer overall confirmation of the null result shown here.

1.5.2 Estimation

This section shows the main results derived from parametric and non-parametric estimations of the (lack of) discontinuities presented in the previous section. First, I estimate equation (1.5) in section 1.4.1 by allowing for a quadratic polynomial of the running variable. The results are presented in table 1.5. As in the previous section, the main outcome variable is the inverse hyperbolic sine transformation of the cumulated money given to municipalities in the years following the elections. The coefficients are all positive but not significant. A large increase in the coefficients' magnitude occurs in the very first year after the elections (2nd column).

These results are robust to different orders of the polynomial function of the running variable. Table 1.6 is a 5x5 matrix in which the coefficients from the 2SLS regressions are shown by year when the outcome variable is measured and by polynomial order. The coefficients are not significant but positive and quite stable across linear, quadratic and cubic functions of the running variable. However, the coefficients for higher polynomials are mostly negative. This finding is in line with the work by Gelman and Imbens (2018) who provide evidence on the noisiness of results from higher order polynomials in RD settings and suggest focusing on lower order polynomials or non-parametric regressions only. Moreover, as the RD plots have shown, the relationship between the running variable and the outcome is smooth and a higher order polynomial is likely to give a bad fit. This is also confirmed by the Akaike Information Criterion (AIC) which is the lowest for linear and quadratic fits[29].

All these results are similar when the outcome is measured as a logarithm or in per capita levels (see tables 1.12 and 1.13 in Appendix 1.B). In addition, as in the graphical analysis, there is no alignment effect either on the extensive or on the intensive margins (see figures 1.23 and 1.24 in Appendix 1.B).

As anticipated in section 1.4.1, I also estimate the discontinuity at the cut-off via non-parametric methods. In particular, I estimate a local linear regression within a narrow bandwidth of the cut-off. For choosing the bandwidth, I follow the current literature (Imbens and Kalyanaraman, 2012; Calonico, Cattaneo, and Titiunik, 2014; Calonico, Cattaneo, and Farrell, 2018). Table 1.7 shows the coefficients from local linear regressions, with three different bandwidths[30]. Panel A and B provide the results based on commonly used bandwidths selected by minimising the MSE. In particular, I use the bandwidth proposed by Calonico, Cattaneo, and Farrell (2018)

[29] Results available upon request.

[30] The reduced form is shown in table 1.14 in Appendix 1.B.

Table 1.5: Quadratic polynomial IV regression: 2SLS and reduced form

	Arcsinh of money allocated by years after election					
	Panel A: 2SLS					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	0.748	1.568	1.675	1.759	1.679	0.742
	(1.072)	(1.079)	(1.119)	(1.144)	(1.124)	(1.096)
Observations	794	794	794	794	794	794
Klebergen-Paap F	617.1	617.1	617.1	617.1	617.1	617.1
	Panel B: Reduced form					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Votes)	0.640	1.341	1.432	1.503	1.435	0.634
	(0.919)	(0.927)	(0.963)	(0.985)	(0.965)	(0.941)
Observations	794	794	794	794	794	794

Notes: the table reports estimates of the alignment effect from the model in equation (1.5) where the polynomial is of order 2. The dependent variable is the inverse hyperbolic sine transformation of the cumulated money defined in different time periods after the elections. The assignment to the treatment, DC wins (Votes), is the instrument for DC winning the elections, DC wins (Seats). The regressions include the running variable, its squared value and their interaction with the treatment dummy. Panel A shows the coefficients from the second stage. Panel B shows the coefficients from the reduced form.The unit of observation is a municipality. Refer to the text for more details on the sample. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

in Panel A and the bandwidth proposed by Imbens and Kalyanaraman (2012) in Panel B. Panel C shows the results based on a bandwidth obtained by minimising the *coverage error* (CER) as suggested by Calonico, Cattaneo, and Titiunik (2014)[31]. The CER approach leads to robust confidence intervals with fastest rate of coverage error decay. Table 1.7 shows how CER-bandwidth selector provides the smallest bandwidth and thus the largest standard errors. Across all three bandwidths, the alignment coefficient is never significant. The results are also insignificant when measuring the outcome in logarithms or at per capita level[32].

Following both CER and MSE methods, the bandwidth choice depends on the outcome being used and so the sample sizes are different in table 1.7. To make the magnitudes of coefficients more comparable, I estimate for each outcome a local

[31]See also Calonico, Cattaneo, and Farrell (2018) and Cattaneo, Idrobo, and Titiunik (2018).
[32]Results available upon request.

Table 1.6: Parametric Regressions by polynomial order

Arcsinh of money allocated by years after election					
up to x years after election	Polynomial order				
	1	2	3	4	5
0	0.261	0.773	0.122	-1.133	-0.817
	(0.808)	(1.130)	(1.478)	(1.778)	(2.098)
1	0.732	1.658	0.593	-1.664	-2.426
	(0.818)	(1.137)	(1.505)	(1.827)	(2.170)
2	0.457	1.758	0.840	-1.206	-2.401
	(0.853)	(1.179)	(1.546)	(1.877)	(2.251)
3	1.092	1.850	1.321	0.142	-1.784
	(0.869)	(1.204)	(1.576)	(1.910)	(2.258)
4	0.967	1.755	1.333	0.129	-1.629
	(0.855)	(1.182)	(1.531)	(1.849)	(2.184)
5	1.091	0.762	1.092	-0.476	-1.665
	(0.831)	(1.153)	(1.497)	(1.800)	(2.130)
Observations	794	794	794	794	794
Klebergen-Paap F	1269	617.1	334.1	232.3	168.2

Notes: the table reports the coefficients from 2SLS regressions as in equation (1.5) for 5 different degrees of polynomial. The dependent variable is the inverse hyperbolic sine transformation of the cumulated money defined in each row as up to x years after the elections. The assignment to the treatment, DC wins (Votes), is the instrument for DC winning the elections, DC wins (Seats). The regressions include the running variable, its squared value and their interaction with the treatment dummy. Each column reports the coefficient obtained from a 2SLS regression with polynomial orders, from 1 to 5, of the running variable. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality. Refer to the text for more details on the sample. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 1.7: Non-parametric Regression: 2SLS

	Arcsinh of money allocated by years after election					
	Panel A: MSE-bandwidth 1					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	0.279	-0.725	-0.683	0.385	0.377	0.0600
	(1.586)	(1.722)	(1.682)	(1.711)	(1.629)	(1.660)
Observations	410	346	364	374	390	384
BW size	0.188	0.147	0.156	0.162	0.170	0.167
	Panel B: MSE-bandwidth 2 (IK)					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	0.186	-0.402	-0.132	0.645	0.173	-0.169
	(1.472)	(1.574)	(1.543)	(1.618)	(1.807)	(1.837)
Observations	448	400	410	406	332	330
BW size	0.230	0.179	0.187	0.181	0.138	0.136
	Panel C: CER-bandwidth					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	-0.628	-0.250	-0.596	0.614	0.274	-0.0875
	(1.827)	(1.999)	(1.961)	(2.017)	(1.935)	(1.970)
Observations	328	256	278	286	298	294
BW size	0.135	0.105	0.112	0.116	0.122	0.120

Notes: the table reports estimates of the alignment effect from a local linear regression. The dependent variable is the inverse hyperbolic sine transformation of the cumulated money defined in different time periods after the elections. The assignment to the treatment, DC wins (Votes), is the instrument for DC winning the elections, DC wins (Seats). Each panel shows the results according to different bandwidths. Panel A and B are based on two types of MSE-minimising bandwidth selectors. Panel C is based on a coverage error-minimising bandwidth selector. The unit of observation is a municipality. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 1.8: Non-parametric Regression: 2SLS - Fixed bandwidth

	Arcsinh of money allocated by years after election					
	Panel A: MSE-bandwidth 1					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	0.276	-0.289	-0.122	0.720	0.622	0.375
	(1.587)	(1.540)	(1.540)	(1.589)	(1.551)	(1.565)
Observations	410	410	410	410	410	410
BW size	0.188	0.188	0.188	0.188	0.188	0.188
	Panel B: MSE-bandwidth 2 (IK)					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	0.187	-0.122	0.0810	0.809	0.764	0.459
	(1.472)	(1.425)	(1.423)	(1.466)	(1.431)	(1.443)
Observations	448	448	448	448	448	448
BW size	0.230	0.179	0.187	0.181	0.138	0.136
	Panel C: CER-bandwidth					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	-0.627	-0.544	-0.653	0.436	0.207	-0.167
	(1.827)	(1.782)	(1.790)	(1.861)	(1.828)	(1.844)
Observations	328	328	328	328	328	328
BW size	0.135	0.135	0.135	0.135	0.135	0.135

Notes: the table reports estimates of the alignment effect from a local linear regression. The dependent variable is the inverse hyperbolic sine transformation of the cumulated money defined in different time periods after the elections. The assignment to the treatment, DC wins (Votes), is the instrument for DC winning the elections, DC wins (Seats). Each panel shows the results according to different bandwidths. The bandwidths are fixed to the level of column (1) in table 1.7. Panel A and B are based on MSE-minimising bandwidth selectors. Panel C is based on a coverage error-minimising bandwidth selector. The unit of observation is a municipality. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

linear regression using the bandwidth used in Column (1) of Panel A which is the bandwidth for the money allocated in the year of the election. Table 1.8 shows the results. The bandwidths are larger than in table 1.7 and this leads to larger and probably more biased coefficients, especially in Panels A and B. Overall, the results confirm that there is no evidence for a significant alignment effect.

1.5.3 Alternative outcomes

The results shown so far provide robust evidence against the existence of an overall alignment effect on the flow of funds received by municipalities in the very first years of CasMez. In this section, I test whether the results change by reproducing the analysis on two alternative measures of the intensity of CasMez intervention: number of projects per capita and average size of projects. Figures 1.7 and 1.8 respectively show the coefficients plots for the parametric regressions for quantity and average size of projects.

For projects per capita the coefficients are mostly positive but not significant. An exception is the effect estimated with a linear polynomial within one year after the election. However, non-parametric estimations provide much smaller and insignificant coefficients across all bandwidth sizes (see table 1.15 in Appendix 1.B). The coefficient for average size of projects is also mostly positive but always insignificant at 95%. Further analysis[33] shows that the magnitude of the standardised coefficients is similar between these two outcomes and the funds allocated and is roughly around 0.2 for polynomials of lower degree, like quadratic or cubic. However, the coefficients are, as shown, mostly insignificant.

1.5.4 Additional robustness checks

The previous sections show that the results are stable across different specifications. In this section I present two additional robustness checks.

First, an important caveat of this analysis is that the electoral system of 1951-52 local elections was *parliamentary*. This means that the elections did not

[33]Results available upon request.

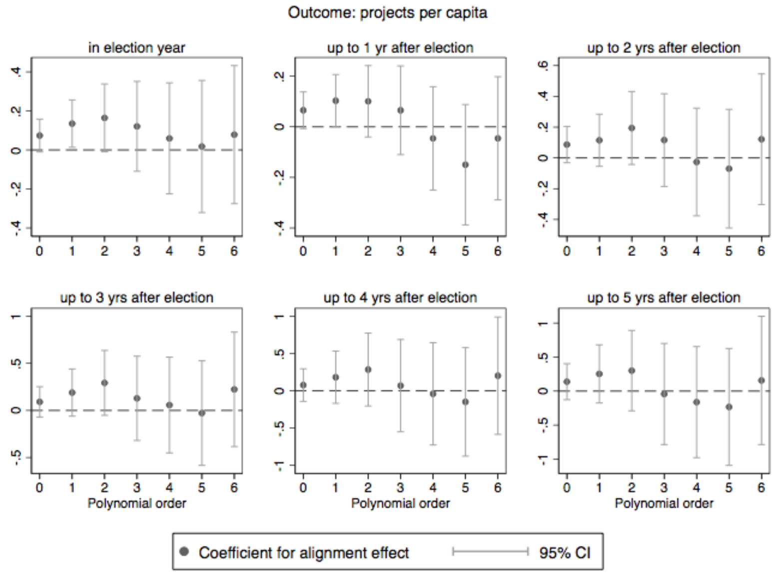


Figure 1.7: **Projects per capita - parametric regressions.** These graphs plot the coefficients of projects per capita, measured in different time periods after the elections, against polynomial orders of the running variable. Projects per capita is the number of projects every 1000 inhabitants, as of 1951 population. The coefficients come from 2SLS regressions where the assignment to the treatment is the instrument for DC winning the elections. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality.

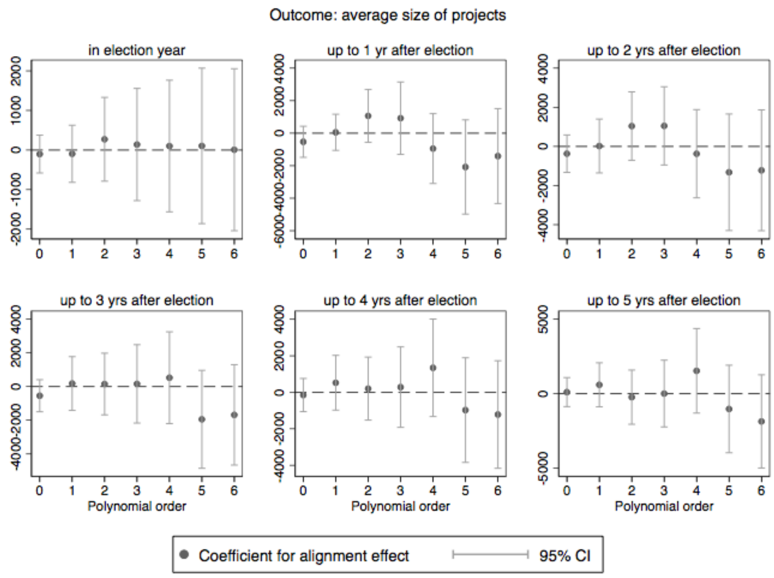


Figure 1.8: **Average size of projects - parametric regressions.** These graphs plot the coefficients of the average size of projects, measured in different time periods after the elections, against polynomial orders of the running variable. The average size of projects is the flow of funds every 100 projects. The coefficients come from 2SLS regressions where the assignment to the treatment is the instrument for DC winning the elections. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality.

directly determine the mayoral incumbent but only the members of the city council only. The mayor was elected through a secret ballot by the council members. The main concern is then that if DC had been able to influence the choice of the mayor ex-post (despite not having the majority of seats) some municipalities might have been aligned with the central government even when DC did not have the majority of the seats. The null treatment effect could then be caused by treating some aligned municipalities as unaligned ones, especially around the cut-off. To test whether the assignment of municipalities to the treatment around the cut-off influences my results, I replicate all the analysis as in Barreca et al. (2011) employing a *donut RDD*. The idea behind this is that by systematically removing observations in the immediate vicinity of the cut-off, I can check whether the estimate of the treatment effect is affected by the observations around the threshold.

Figure 1.9 shows the treatment estimate from a parametric regression with a quadratic polynomial function of the running variable by different bandwidths dropped around the cut-off. The first coefficients in each graph are the ones shown in table 1.5. The identification in a RDD is intrinsically related to the observations around the cut-off so that estimates obtained when excluding them are highly biased and misleading. In fact, the larger the number of observations dropped around the cut-off the larger are the coefficients, especially in the first two years after the elections. Also, the coefficients gain some statistical significance once the observations within the [-0.4;0-4] bandwidth are dropped which leads the sample size to drop by 20%. Overall, I conclude that for reasonable bandwidths around the cut-off the coefficients for the alignment effect is stable and never statistically significant.

Finally, as described in section 2.3, CasMez was funding, to a large extent, *multi-municipality* projects. To check that results are not determined by the way I assign multi-municipality funds to single municipalities, I replicate the analysis on single-municipality projects only. The results[34] are very similar to the ones presented in the previous sections.

[34] Available upon request.

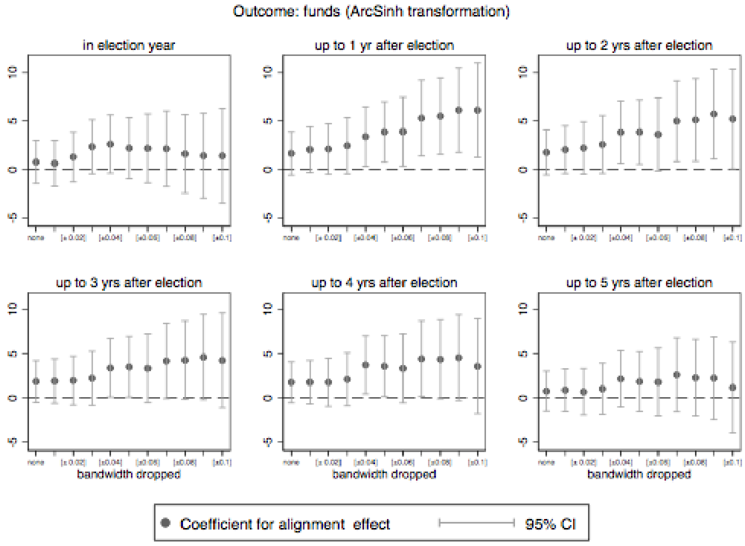


Figure 1.9: **Donut RDD** These graphs plot the coefficients of parametric regressions with a quadratic polynomial of the running variable against the bandwidths within which observations have been dropped. The outcome is the inverse hyperbolic sine transformation of the cumulated money allocated to municipalities defined in different time periods after the elections. The coefficients come from 2SLS regressions where the assignment to the treatment is the instrument for DC winning the elections. The unit of observation is a municipality.

1.6 Mechanisms and discussion

The results in section 2.5 show that the allocation of CasMez investments is not significantly affected by the political alignment of municipalities with the central government. In this section I provide some further findings suggesting that there is some evidence for tactical distribution, but the effect is heterogeneous and does not to show up at the aggregate level. First, on the basis of the ideological roots of CasMez described in section 1.2 I look at whether *alignment to the main opposition party* (PCI) mattered in the allocation of funds. Second, I explore some dimensions of heterogeneity among municipalities and test related predictions of the literature on distributive politics.

1.6.1 Alignment with main opposition party

CasMez was part of a declared political strategy of both the US and the Italian governments to support the development of southern regions but to curb anti-governmental and especially communist sentiments(WB, 1951). Moreover, the local elections of 1951-52 were very relevant for testing the increasing support for the communist party (PCI) (Possanzini, 2000). In this context, the results of PCI rather than of DC in these elections might have played a more *direct* role in the distribution of funds. To test this, I employ an empirical setting that is identical to the one in the main analysis, but I reframe it to identify a specific form of un-alignment, which is alignment to the *main opposition party.*

In order to do this, I look at the sample of all municipalities where PCI obtained the largest or second-largest share of votes[35]. The running variable is the votes' margin of victory (VMV) which is the difference in shares of votes between PCI and the main opponent, and is defined as negative when PCI is the second party. In this framework, municipalities above the cut-off are *unaligned* with the central government while the ones below are *aligned.* As in the rest of the paper, the probability of party control is however not exactly equal to 1 when the running variable is equal to 0; so I use the assignment to the treatment as an instrument for the treatment status of alignment to main opposition party. As in section 2.5, this setup is invalid if there is manipulation of the running variable. Figures 1.25 and 1.26 in Appendix 1.B.3 show both the density distribution and the RD plots for covariates. Both the tests proposed by (McCrary, 2008) and Cattaneo, Jansson, and Ma (2017) cannot reject the null of no density discontinuity at the cut-off. The RD plots suggest that municipalities where PCI just won are larger, more active in agriculture and have more jobs per capita. These differences are not significant[36], but must be borne in mind when interpreting the results.

Figure 1.10 shows the RD plots for the main outcome, the money allocated in the years following the election. The outcome is on average higher to the right of the cut-off, in municipalities where PCI just won. Figure 1.11 shows the estimated discontinuity by order of polynomial in parametric regressions. The coefficients are strongly positive and significant for higher order polynomials when the outcome is the money allocated up to four or five years subsequent to the election.

[35]For summary statistics of this sample refer to table 1.16 in Appendix 1.B.3
[36]Results from placebo regressions are available upon request.

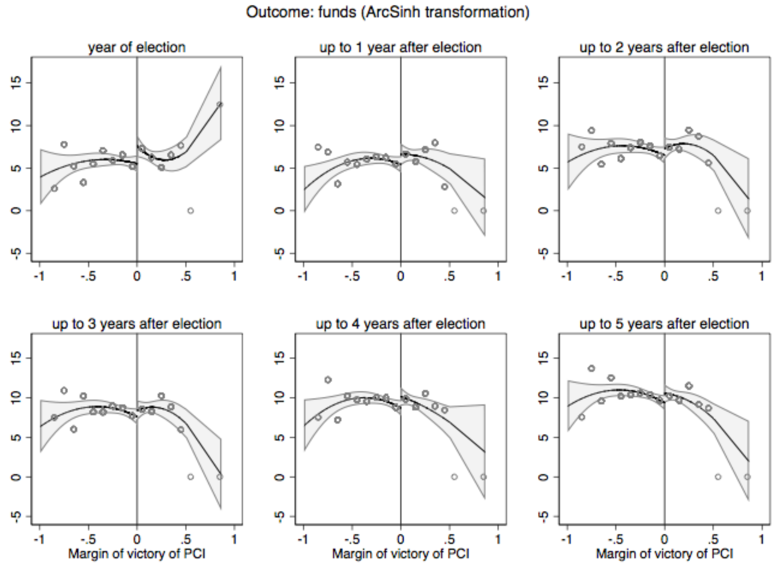


Figure 1.10: **Inverse hyperbolic sine transformation of funds allocated against votes' margin of victory (VMV) of PCI by years after the election - PCI races.** These graphs show the average money allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between PCI and the main opponent party or coalition. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

These results are also confirmed by non-parametric regressions. As table 1.9 shows, the coefficients are large and positive across all three optimal bandwidths but consistently significant only in columns (5) and (6).

In terms of magnitude, the coefficients are extremely large and definitely off what has been found so far in the literature. However, this result is likely to be driven by the fact that there are many zeroes in the sample, as more than half of the municipalities did not receive any funds from CasMez within the first year from the election. To test this, I look at whether the results are stronger on the extensive than on the intensive margin. Figures 1.27 and 1.28 in Appendix 1.B.3 confirm this interpretation by showing that the effect is significant 4 years after the election on the extensive but not on the intensive margin.

These results indicate the presence of an alignment to rival party at the end of the term for the city councils. Municipalities where PCI just won are more likely to be targeted by CasMez than municipalities where it just lost in the year of

Table 1.9: Non-parametric Regression: 2SLS - PCI races

	Arcsinh of money allocated by years after election					
	Panel A: MSE-bandwidth 1					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
PCI wins (Seats)	3.669*	3.700*	3.875*	3.114	5.444***	4.884**
	(2.086)	(1.920)	(2.087)	(2.038)	(2.079)	(2.057)
Observations	382	414	392	414	348	336
BW size	0.120	0.129	0.124	0.129	0.107	0.104
	Panel B: MSE-bandwidth 2 (IK)					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
PCI wins (Seats)	3.252**	3.250**	2.043	2.322	4.883***	3.574**
	(1.535)	(1.610)	(1.340)	(1.625)	(1.791)	(1.601)
Observations	622	528	720	542	452	490
BW size	0.233	0.176	0.296	0.192	0.141	0.160
	Panel C: CER-bandwidth					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
PCI wins (Seats)	3.810	3.450	3.830	3.183	6.521**	5.591**
	(2.640)	(2.405)	(2.676)	(2.565)	(2.823)	(2.755)
Observations	272	292	282	290	232	224
BW size	0.0859	0.0929	0.0890	0.0925	0.0770	0.0749

Notes: the table reports estimates of the alignment to opposition effect from a local linear regression. The dependent variable is the inverse hyperbolic sine transformation of the cumulated money defined in different time periods after the elections. The assignment to the treatment, PCI wins (Votes), is the instrument for PCI winning the elections, PCI wins (Seats). Each panel shows the results according to different bandwidths. Panel A and B are based on two types of MSE-minimising bandwidth selectors. Panel C is based on a coverage error-minimising bandwidth selector. The unit of observation is a municipality. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

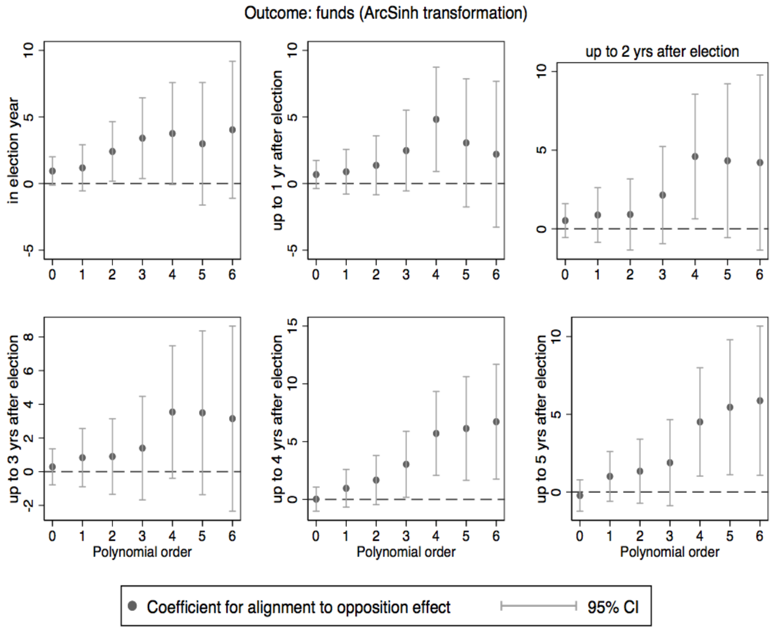


Figure 1.11: **Inverse hyperbolic sine of money allocated by years after election - parametric regressions - PCI races.** These graphs plot the coefficients of the money allocated, measured in different time periods after the elections, against polynomial orders of the running variable, that is VMV for PCI. The coefficients come from 2SLS regressions where the assignment to the treatment is the instrument for PCI winning the elections. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality.

the next local elections. A potential explanation for these findings is that DC was attempting to swing marginal constituencies away from PCI. Other scholars have also found that politicians use transfers for political reasons especially in election years (for example see Brollo and Nannicini, 2012).

However, there are some limitations to these findings. First, the results are not very stable across different polynomial orders or bandwidths. Second, as some observables are not smooth around the cut-off, the discontinuity in funds at the cut-off might be driven by economic, and not political, considerations. Data limitations do not allow me to address these two points. Finally, these results are not really comparable to the ones presented in the main analysis as the samples differ. In fact, there are only 271 elections in which DC without coalition and PCI directly faced each other. To make the results comparable, one possibility would be to replicate the analysis on these DC-PCI races only. However, figure 1.29 in Appendix 1.B.3

shows that the density distribution of the running variable in this subsample jumps up at the cut-off. Unsurprisingly, there is evidence for manipulation in very tight electoral races. This and also the small sample size do not allow further investigation of the relationship between CasMez funds and alignment in a RDD setting.

1.6.2 Heterogeneity analysis

In this section I reconsider the baseline null results found in DC races by exploiting the heterogeneity in municipality characteristics. To do so, I split the sample into groups of municipalities above and below the median of some observables of interest. Then, I check if the assumptions of RDD are still valid in these subsamples and, if so, I estimate the alignment effect.

First of all, given the findings on PCI races, I test whether the baseline results change in agricultural areas. As described in section 1.2, peasants' strikes and lands' invasions supported by PCI had a crucial role in pushing DC to actively intervene on the development of the South (Crafts and Magnani, 2013). To test this, I split the sample into *rural* and *non-rural* areas, according to the median of the share of residents active in agriculture. Panel A of table 1.10 shows the results of the non-parametric regressions where the outcome is again the money allocated by years after election and the bandwidth used is the MSE-optimal bandwidth proposed by Calonico, Cattaneo, and Farrell (2018)[37]. Interestingly, the coefficients are positive in non-rural areas, below the median of agricultural activity, but negative in rural areas. Most importantly, distinguishably from the baseline results, the coefficients gain statistical significance within two years from the election.

As in section 1.6.1, these coefficients are very large and hard to interpret. As Panel B shows, this is due to a large effect of alignment on the extensive margin. In rural areas, municipalities where DC just won are 35% *less* likely to be targeted by CasMez with respect to municipalities where DC just lost. In contrast, non-rural municipalities were DC just won are 25% *more* likely to receive CasMez funds. All these results are confirmed by parametric regressions[38].

[37]Results are similar for other bandwidths and available upon request.

[38]Figure 1.30 in section 1.B.4 shows the coefficients plots for the parametric regressions in the

Overall, these findings indicate that the absence of an overall alignment effect can be explained by the co-existence of both alignment and un-alignment effects, depending on the type of constituencies targeted. A natural way of interpreting these results is to say that in non-rural or urban areas DC used the funds from CasMez to reward its supporters or build party strongholds, as in a partisan alignment model. Yet, in rural areas that were *at risk* because of exposure to greater political instability, DC used the funds to swing marginal municipalities away from the opposition. These results are also in line with the findings presented in section 1.6.1 which show that municipalities aligned with PCI received on average more funds.

Another possibility is that these results are driven by other municipality characteristics that are correlated with agricultural activity. In particular, one of the predictions of the swing-voter model in the distributive politics literature is that parties can increase their support by targeting voters who can be *bought off* with a relative low flow of transfers. In particular, Dixit and Londregan (1996) show how it can be more efficient for politicians to transfer resources to poorer voters. Similarly, Grossman and Helpman (1996) consider the difference between informed and uninformed voters and predict that uninformed voters are easier to impress and thus *cheaper* to buy. These predictions applied to the context of political alignment and no political credit spillovers suggest that unaligned rural municipalities might get more transfers because they are cheaper to buy off (and not more politically unstable) than unaligned non-rural municipalities.

To test this possibility, I look at whether the alignment coefficient differs between municipalities above and below the median of: number of households with no access to water and electricity as a proxy for pre-industrial development, and illiteracy rate as a proxy for rate of uninformed voters. Table 1.11 shows the results from non-parametric regressions with the MSE-optimal bandwidth proposed by Calonico, Cattaneo, and Farrell (2018)[39]. The signs of the coefficients are completely opposite with respect to the pattern found for non-agricultural and agricultural areas. The un-alignment effect comes from relatively more educated and richer areas.

two subsamples.

[39]Results are similar for other bandwidths and available upon request.

Table 1.10: Heterogeneity analysis I: rural areas

	Split by % of residents active in agriculture											
	Panel A: Arcsinh of money allocated by years after election											
	Below median						Above median					
Up to x years after election	0	1	2	3	4	5	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DC wins (Seats)	0.466	2.954*	3.627**	2.979	1.879	1.070	0.215	-4.946*	-4.667*	-2.355	-1.533	-1.308
	(2.025)	(1.712)	(1.824)	(2.162)	(2.068)	(2.118)	(2.296)	(2.637)	(2.493)	(2.515)	(2.430)	(2.514)
Observations	244	244	226	202	204	202	202	184	198	198	202	198
BW size	0.224	0.227	0.196	0.167	0.167	0.165	0.218	0.175	0.201	0.202	0.212	0.201
	Panel B: Dummy for recipients											
	Below median						Above median					
Up to x years after election	0	1	2	3	4	5	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DC wins (Seats)	-0.0606	0.247*	0.276**	0.249	0.135	0.0747	0.0163	-0.343*	-0.352*	-0.160	-0.0934	-0.0707
	(0.163)	(0.135)	(0.139)	(0.163)	(0.152)	(0.156)	(0.174)	(0.194)	(0.185)	(0.184)	(0.175)	(0.178)
Observations	226	228	222	204	204	202	202	186	196	192	198	190
BW size	0.192	0.206	0.188	0.167	0.167	0.163	0.219	0.177	0.196	0.195	0.204	0.194

Notes: the table reports estimates of the treatment effect from a local linear regression. The dependent variables are the inverse hyperbolic sine transformation of the cumulated money in Panel A and a dummy for receiving at least one in Panel B. Both variables are defined in different time periods after the elections. The coefficients are reported for both outcomes from two subsamples obtained by splitting municipalities according to the median of % residents active in agriculture. The bandwidth is the optimal MSE-minimising bandwidth.The unit of observation is a municipality. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 1.11: Heterogeneity analysis II: *cheaper* voters

	Arcsinh of money allocated by years after election											
	Panel A: Illiteracy rate											
	Below median						Above median					
Up to x years after election	0	1	2	3	4	5	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DC wins (Seats)	-0.284	-0.603	-1.963	-0.955	-1.516	-2.209	0.761	0.00620	1.764	2.213	2.319	2.662
	(1.840)	(2.180)	(2.137)	(2.222)	(1.985)	(1.990)	(1.963)	(2.143)	(2.078)	(2.033)	(1.992)	(2.012)
Observations	186	180	180	178	178	178	240	240	242	242	240	244
BW size	0.200	0.191	0.192	0.188	0.182	0.189	0.216	0.218	0.223	0.223	0.219	0.234
	Panel B: % of households with no water/electricity											
	Below median						Above median					
Up to x years after election	0	1	2	3	4	5	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DC wins (Seats)	-2.445	-2.328	-2.089	-2.328	-2.933	-2.376	1.581	0.568	1.845	3.265	3.482	1.922
	(2.146)	(2.665)	(2.644)	(2.864)	(2.371)	(2.700)	(2.581)	(2.126)	(1.953)	(2.025)	(2.135)	(2.324)
Observations	238	204	214	206	226	206	164	190	208	208	192	174
BW size	0.275	0.190	0.224	0.209	0.253	0.203	0.125	0.156	0.185	0.185	0.158	0.140

Notes: the table reports estimates of the treatment effect from a local linear regression. The dependent variable is the inverse hyperbolic sine transformation of the cumulated money defined in different time periods after the elections. Each panel shows the results according to different splits of municipality characteristics. The bandwidth is the optimal MSE-minimising bandwidth.The unit of observation is a municipality. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Most importantly, these coefficients are not significant. This evidence supports the existence of *political instability* rather than *poverty* mechanism behind the un-alignment effect in agricultural areas[40].

Overall these findings do not provide a clear-cut picture of all the political factors behind the allocation of funds but show that the overall null effect of political alignment masks an important heterogeneity. In particular, this analysis indicates the co-existence of both alignment and un-alignment effects depending on the degree of political instability of the targeted constituencies. When unaligned marginal constituencies cannot benefit from political credit spillover of transfers *and* are exposed to political instability, the central government can use the transfers to buy their votes.

1.7 Conclusion

A large body of research in economics and political science has empirically documented a positive effect of political alignment on the distribution of central governments funds to local jurisdictions. The theoretical foundations for the direction of this effect are crucially related to the opportunity for unaligned local governments to claim partial or full political credit for the transfers. This work, to the best of my knowledge, is the first to study the link between political alignment and the transfers of an unmistakably *government-branded* development policy.

In particular, I investigate whether the alignment of municipalities with the central government influenced the initial allocation of infrastructural projects by *Cassa del Mezzogiorno*, a large place-based policy in post-WWII Italy. CasMez makes a particularly insightful case study, especially because of its deep ideological roots. It was unambiguously a policy of the ruling party (DC) and was openly criticised by the main opposition party (PCI). This rules out the presence of political credit spillovers. In addition, the alignment is measured on the basis of local elections that were politically very salient and this contributes to providing an ideal context

[40] Table 1.17 in Appendix 1.B.4 shows also that this effect is not driven by densely populated areas or with higher turnout.

for testing distributive politics.

Using an RDD, I find no difference in the amount of CasMez resources received between municipalities where DC just won and municipalities where DC just lost. Further analysis, however, shows that this null result covers the coexistence of both alignment and un-alignment effects. In particular, I find evidence that rural municipalities were more likely to be targeted by CasMez if DC just lost, while the opposite held true for non-rural or urban municipalities. These findings are consistent with well-documented concern by DC regarding the increasing peasants' uprisings in agricultural areas. In line with this, I also show that municipalities where PCI just lost received more fundings than municipalities where PCI just won.

This paper makes two main contributions. It adds to the literature on the effects of political partisanship by highlighting the importance of two crucial factors for the relationship between alignment and the allocation of funds. The first is the possibility of an un-alignment effect when opposition parties cannot take credit for the transfers. In this case, the central government can use transfers to try to shift votes away from the opposition in marginal unaligned municipalities. The second factor is that heterogeneity in the political context of local jurisdictions may change the direction of the allocation mechanism. In my context, the degree of political instability may have determined the decision of the central government to target aligned or unaligned municipalities.

This paper also contributes to the growing literature on CasMez by providing the first quantitative investigation of its allocation mechanisms. I provide evidence that some tactical considerations played a role for the funds' distribution during the very first years of its activity. These findings are in line with the ideological roots of this intervention but in contrast with the common view that the first years of CasMez were an example of politically independent intervention in comparison with its later phases.

1.A CasMez

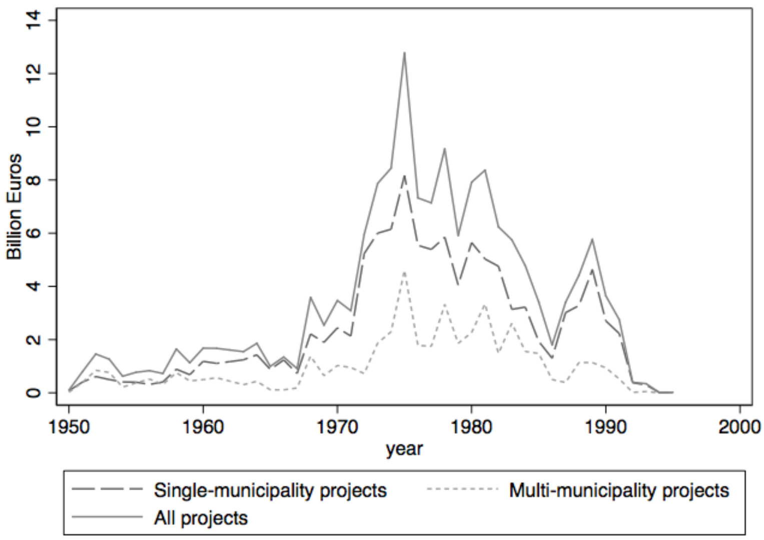


Figure 1.12: **CasMez money over time, with and without multi-municipality projects**. The money in each year is measured as any money allocated during that calendar year. Multi-municipality projects are projects involving more than one municipality.

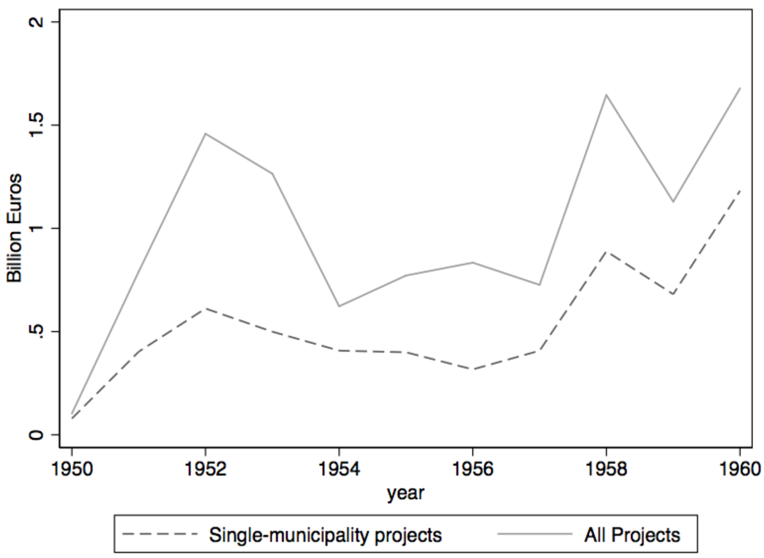


Figure 1.13: **CasMez money over time, in the first decade of activity, with and without multi-municipality projects**. The money in each year is measured as any money allocated during that calendar year. Multi-municipality projects are projects involving more than one municipality.

1.A.1 Local elections

PROVINCIA DI NAPOLI 793

Comune di ACERRA

Elezione del Consiglio Comunale

del 31 marzo 1946 (V.L.) Elettori: 12.518 — Votanti: 79,0 %	Voti in testa teorici	%	Seggi	del 25 maggio 1952 (L.C.) Elettori: 13.923 — Votanti: 90,5 %	Voti di lista e di *gruppo*	%	Seggi
S.C.	2.586	*27,8*	6	**P.C.I.**	2.703	*22,2*	9
				P.S.I.	2.806	*23,1*	10
				I.S.	199	*1,6*	1
					5.708		
D.C.	5.562	*59,8*	24	**D.C.**	5.399	*44,3*	8
Dem. Lib. (1) .	706	*7,6*	—				
Ind.	451	*4,8*	—	**M.S.I.**	1.074	*8,8*	2
	9.305	***100,0***	**30**		**12.181**	***100,0***	**30**

(1) Democrazia Liberale.

Elezione del Consiglio Provinciale

(25 maggio 1952)

Collegio di ACERRA

Elettori: 13.923 — Votanti: 90,5 %

		Voti validi ai candidati	%
Tromba	– **I.S. (S.C.)**	6.611	*54,9*
Scudo crociato	– **D.C.**	4.381	*36,4*
Bandiera	– **P.L.I.**	122	*1,0*
Fiamma	– **M.S.I.** (1)	923	*7,7*
		12.037	***100,0***

(1) Collegato con P.N.M.

12

Figure 1.14: **Sample of data for local election results in a municipality**

1.B Additional results

1.B.1 Graphical analysis

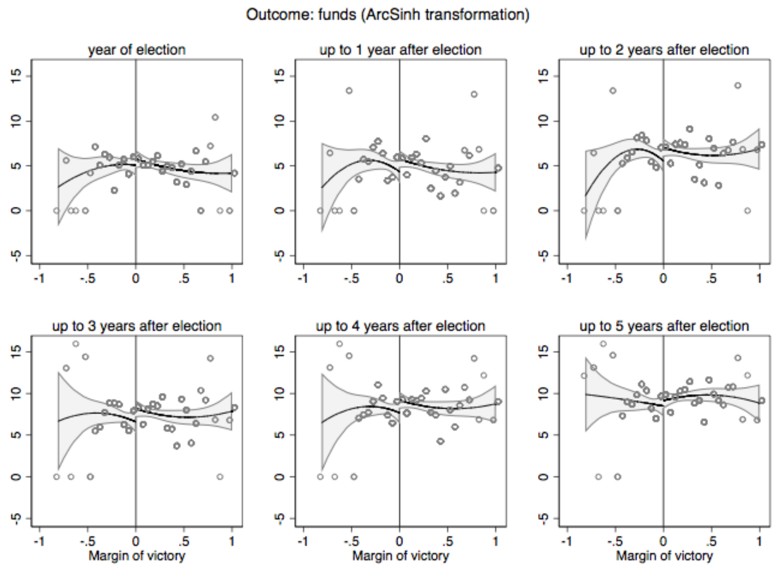


Figure 1.15: **Inverse hyperbolic sine transformation of cumulated money against votes' margin of victory (VMV) by years after the election - bin size 5%** These graphs show the average money allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition. The circles represent means within 5% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

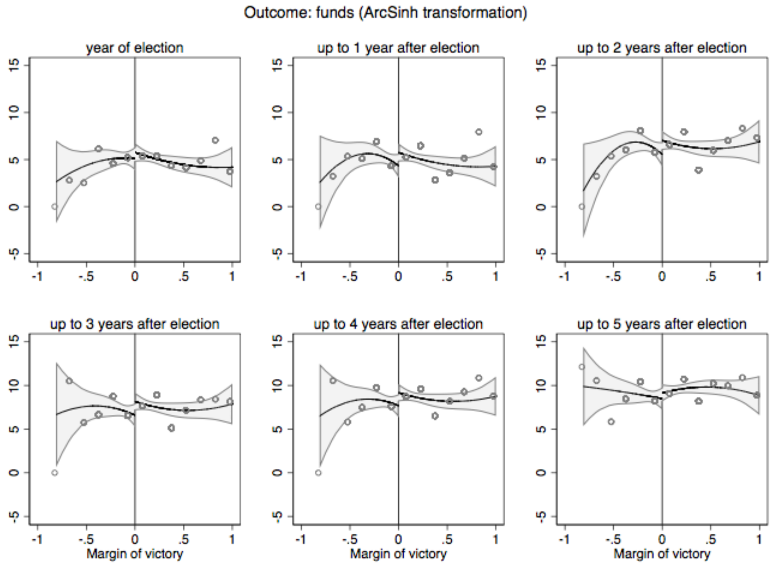


Figure 1.16: **Inverse hyperbolic sine transformation of cumulated money against votes' margin of victory (VMV) by years after the election - bin size 15%** These graphs show the average money allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition. The circles represent means within 15% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

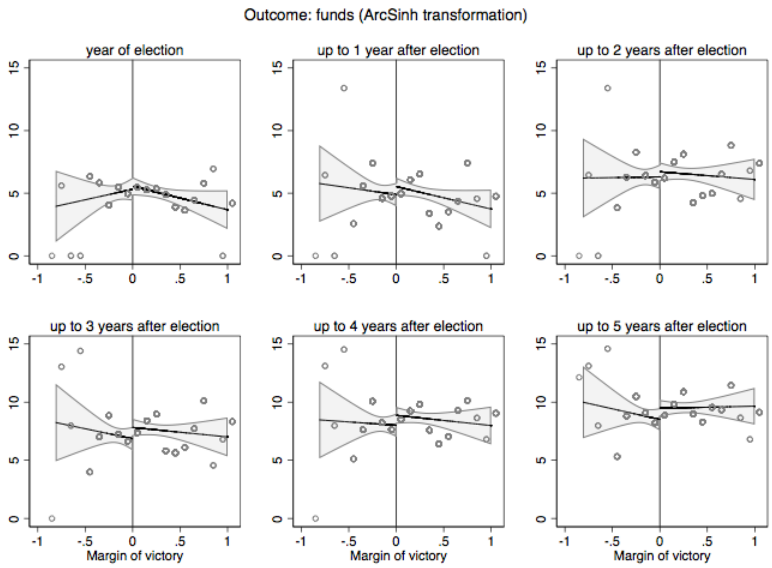


Figure 1.17: **Inverse hyperbolic sine transformation of cumulated money against votes' margin of victory (VMV) by years after the election - linear polynomial** These graphs show the average money allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a linear regression on each side of the cut-off.

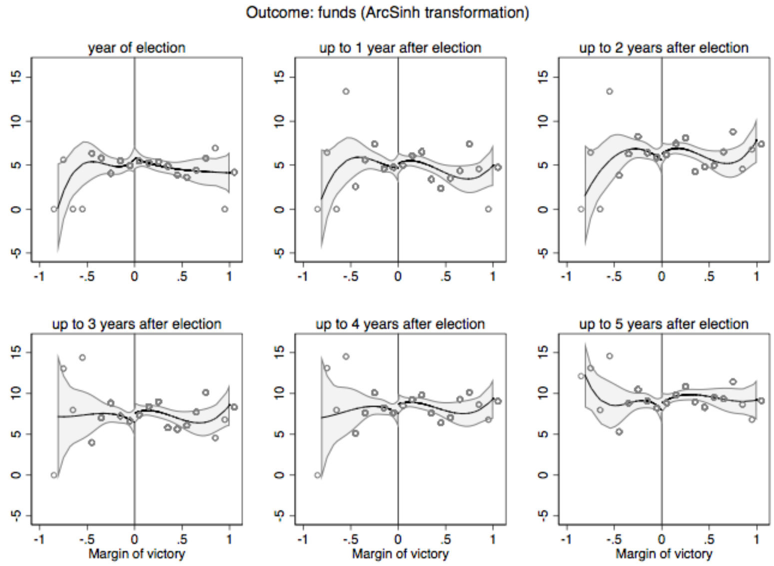


Figure 1.18: **Inverse hyperbolic sine transformation of cumulated money allocated against votes' margin of victory (VMV) by years after the election - cubic polynomial** These graphs show the average money allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a cubic polynomial regression on each side of the cut-off.

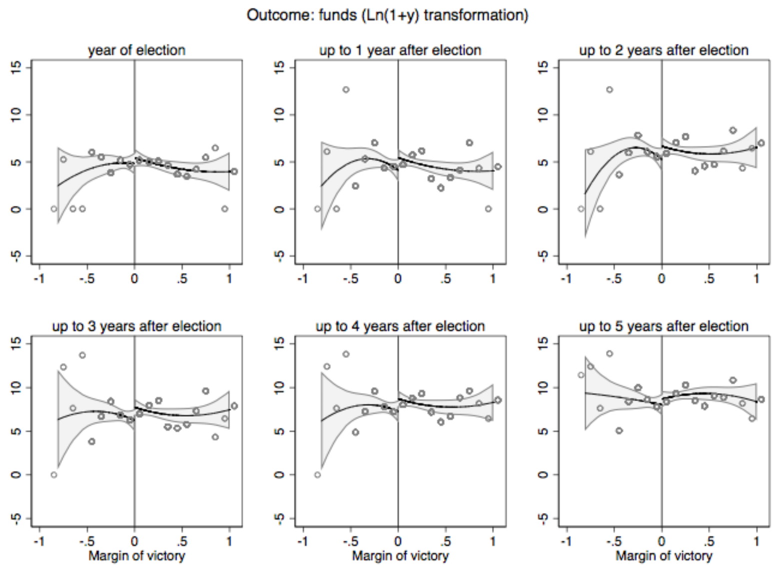


Figure 1.19: **Log of cumulated money allocated against votes' margin of victory (VMV) by years after the election - ln(1+y)** These graphs show the average log of money allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

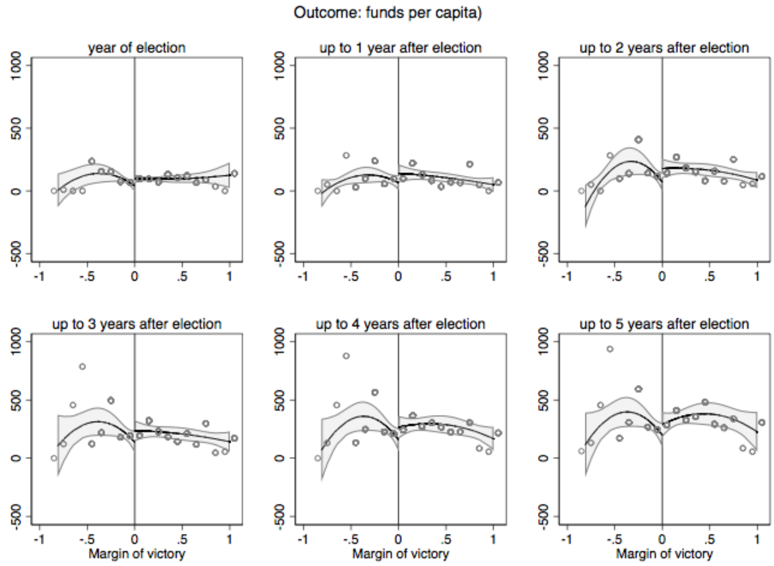


Figure 1.20: **Cumulated money per capita allocated against votes' margin of victory (VMV) by years after the election** These graphs show the average money per capita allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

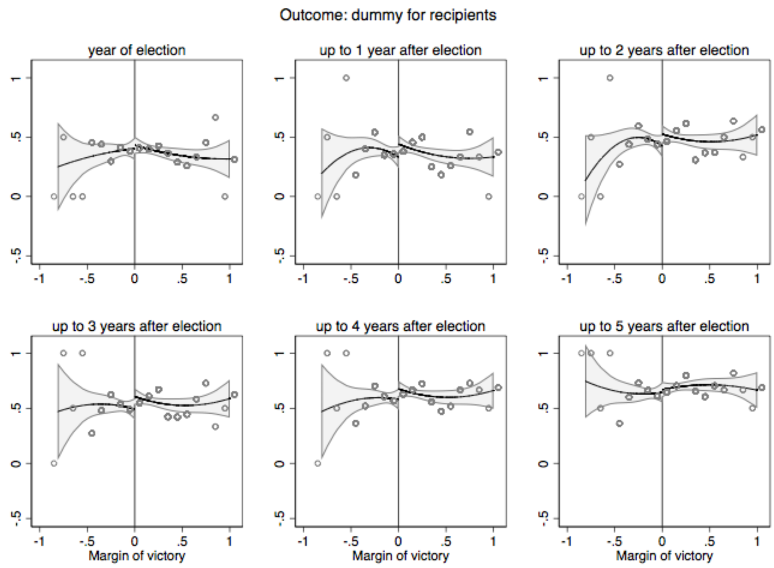


Figure 1.21: **Extensive margin. Share of municipalities that received at least one grant against votes' margin of victory (VMV) by year after the elections.** These graphs plot a dummy for municipalities that received at least one grant, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition.The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

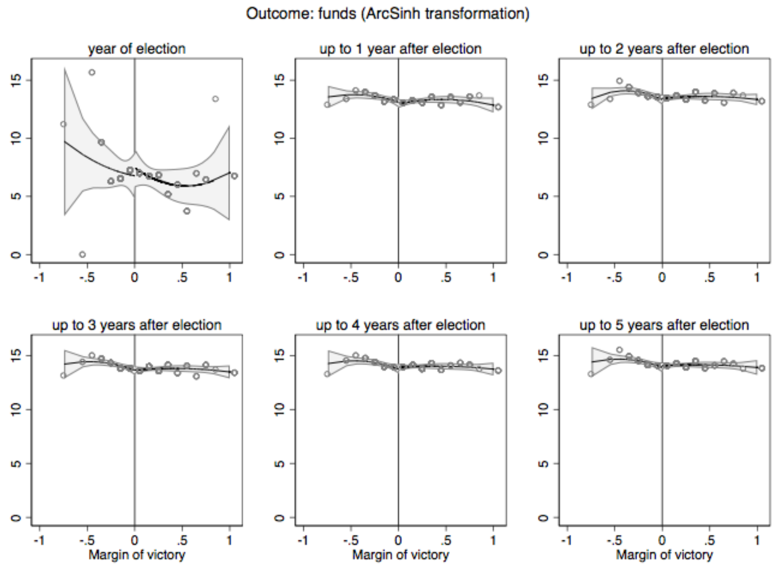


Figure 1.22: **Intensive margin. Inverse hyperbolic sine transformation of funds allocated against votes' margin of victory (VMV) by years after the election - Recipients since year 1 only.** These graphs show the average money allocated to municipalities, defined in different time periods after the elections, conditional on the difference in the share of votes between DC and the main opponent party or coalition. Sample is restricted to municipalities that received at least one grant within the first year from the elections. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

1.B.2 Baseline results

Table 1.12: Parametric Regressions by polynomial order

Log of money allocated by years after election					
up to x years after election	Polynomial order				
	1	2	3	4	5
0	0.252	0.748	0.139	-1.036	-0.741
	(0.766)	(1.072)	(1.401)	(1.685)	(1.988)
1	0.691	1.568	0.553	-1.596	-2.319
	(0.776)	(1.079)	(1.428)	(1.733)	(2.058)
2	0.436	1.675	0.800	-1.150	-2.280
	(0.810)	(1.119)	(1.468)	(1.782)	(2.137)
3	1.037	1.759	1.244	0.117	-1.715
	(0.826)	(1.144)	(1.498)	(1.815)	(2.146)
4	0.927	1.679	1.271	0.117	-1.560
	(0.813)	(1.124)	(1.456)	(1.758)	(2.076)
5	1.046	0.742	1.040	-0.456	-1.600
	(0.790)	(1.096)	(1.424)	(1.713)	(2.027)
Observations	794	794	794	794	794
IV F-stat	1269	617.1	334.1	232.3	168.2

Notes: the table reports the coefficients from 2SLS regressions as in equation (1.5) for 5 different degrees of polynomial. The dependent variable is the logarithmic transformation of the cumulated money defined in each row as up to x years after the elections. The assignment to the treatment, DC wins (Votes), is the instrument for DC winning the elections, DC wins (Seats). The regressions include the running variable, its squared value and their interaction with the treatment dummy. Each column reports the coefficient obtained from a 2SLS regression with polynomial orders, from 1 to 5, of the running variable. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality. Refer to the text for more details on the sample. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 1.13: Parametric Regressions by polynomial order

Money per capita allocated by years after election					
up to x years after election	Polynomial order				
	1	2	3	4	5
0	15.17	65.340021*	29.79	36.01	21.86
	(24.684)	(39.187)	(52.922)	(60.798)	(75.354)
1	58.98	86.44	50.47	-47.17	-81.33
	(52.015)	(66.894)	(74.831)	(81.401)	(109.667)
2	61.48	126.2	108.7	-1.550	-71.34
	(58.120)	(77.343)	(84.977)	(87.933)	(126.638)
3	61.78	113.3	70.03	3.846	-111.9
	(69.213)	(93.801)	(110.269)	(119.675)	(153.674)
4	81.79	126.1	98.50	78.99	-42.16
	(72.250)	(99.316)	(117.385)	(127.194)	(160.841)
5	86.16	104.3	56.99	66.62	-82.64
	(76.195)	(107.041)	(128.327)	(147.069)	(184.209)
Observations	794	794	794	794	794
IV F-stat	1269	617.1	334.1	232.3	168.2

Notes: the table reports the coefficients from 2SLS regressions as in equation (1.5) for 5 different degrees of polynomial. The dependent variable is the cumulated money per capita (population of 1951) defined in each row as up to x years after the elections. The assignment to the treatment, DC wins (Votes), is the instrument for DC winning the elections, DC wins (Seats). The regressions include the running variable, its squared value and their interaction with the treatment dummy. Each column reports the coefficient obtained from a 2SLS regression with polynomial orders, from 1 to 5, of the running variable. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality. Refer to the text for more details on the sample. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 1.14: Non-parametric Regression: Reduced form

	Arcsinh of money allocated by years after election					
	Panel A: MSE-bandwidth 1					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Votes)	0.187	-0.529	-0.566	0.292	0.176	-0.145
	(1.382)	(1.523)	(1.549)	(1.573)	(1.578)	(1.596)
Observations	408	332	328	342	330	328
BW size	0.183	0.139	0.135	0.142	0.136	0.135
	Panel B: MSE-bandwidth 2 (IK)					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Votes)	0.160	-0.346	-0.113	0.556	0.150	-0.146
	(1.266)	(1.354)	(1.330)	(1.393)	(1.563)	(1.591)
Observations	448	400	410	406	332	330
BW size	0.230	0.179	0.187	0.181	0.138	0.136
	Panel C: CER-bandwidth					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Votes)	-0.573	-0.188	-0.501	0.673	0.600	0.136
	(1.610)	(1.793)	(1.842)	(1.891)	(1.927)	(1.946)
Observations	794	794	234	794	238	794
Observations	320	246	794	248	794	234
BW size	0.131	0.0993	0.0967	0.101	0.0973	0.0969

Notes: the table reports reduced form estimates of the assignment to treatment effect from a local linear regression. The dependent variable is the inverse hyperbolic sine transformation of the cumulated money defined in different time periods after the elections. Each panel shows the results according to different bandwidths. Panel A and B are based on two types of MSE-minimising bandwidth selectors. Panel C is based on a coverage error-minimising bandwidth selector. The unit of observation is a municipality. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 1.15: Non-parametric Regression: 2SLS

	Projects per capita by years after election					
	Panel A: MSE-bandwidth 1					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	0.0939	-0.0301	0.0962	0.170	0.133	0.0627
	(0.142)	(0.0918)	(0.159)	(0.233)	(0.311)	(0.375)
Observations	380	344	390	408	400	410
BW size	0.165	0.144	0.170	0.182	0.180	0.188
	Panel B: MSE-bandwidth 2 (IK)					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	0.141	-0.00926	0.0855	0.160	0.117	0.0447
	(0.0895)	(0.0885)	(0.161)	(0.238)	(0.315)	(0.381)
Observations	562	394	372	386	380	392
BW size	0.436	0.174	0.161	0.168	0.165	0.172
	Panel C: CER-bandwidth					
Up to x years after election	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)
DC wins (Seats)	0.0615	0.0119	0.125	0.174	0.136	0.00708
	(0.162)	(0.0976)	(0.169)	(0.249)	(0.316)	(0.384)
Observations	292	250	298	318	318	328
BW size	0.118	0.103	0.122	0.130	0.129	0.135

Notes: the table reports estimates of the treatment effect from a local linear regression. The dependent variable is the number of projects every 1000 inhabitants in different time periods after the elections. Each panel shows the results according to different bandwidths. Panel A and B are based on two types of MSE-minimising bandwidth selectors. Panel C is based on a coverage error-minimising bandwidth selector. The unit of observation is a municipality. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

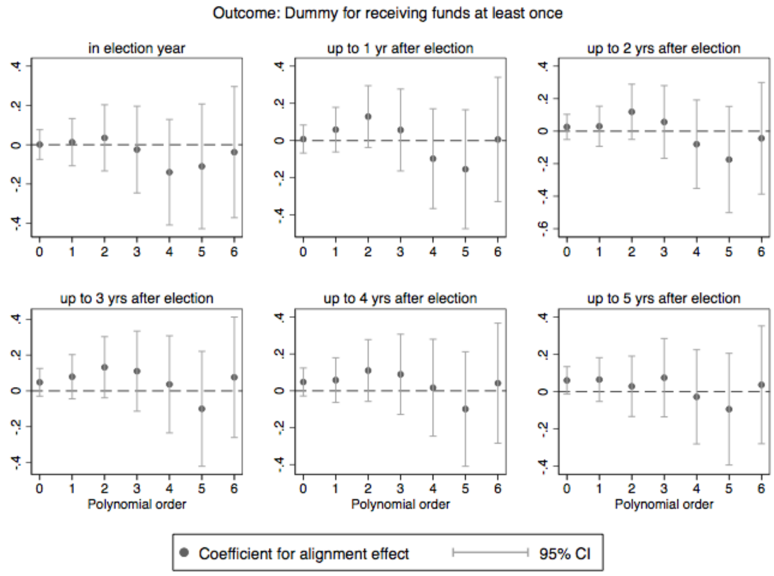


Figure 1.23: **Extensive margin - parametric regressions.** These graphs plot the coefficients of a dummy equal to 1 if the municipalities received at least one transfer, measured in different time periods after the elections, against polynomial orders of the running variable. The coefficients come from 2SLS regressions where the assignment to the treatment is the instrument for DC winning the elections. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality.

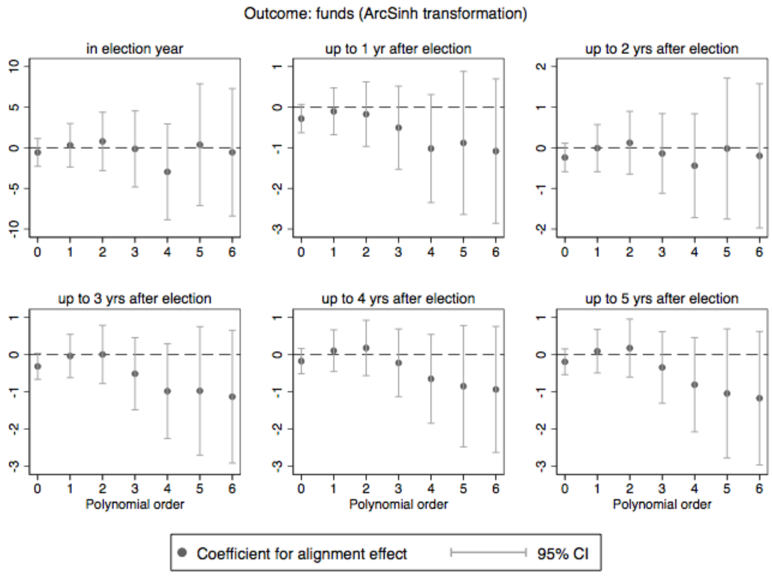


Figure 1.24: **Inverse hyperbolic sine of money allocated by years after election - parametric regressions - intensive margin.** These graphs plot the coefficients of the money allocated, measured in different time periods after the elections, against polynomial orders of the running variable . The sample consists of municipalities that received CasMez money at least once within one year from the election. The coefficients come from 2SLS regressions where the assignment to the treatment is the instrument for DC winning the elections. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality.

1.B.3 PCI races

Table 1.16: Summary statistics (PCI races)

Variable	Mean	Std. Dev.	Min.	Max.	N
Dummy if targeted by CasMez	0.68	0.47	0	1	766
Log of money within 4 years	9.41	6.61	0	18.66	766
Number of projects every 1000 inhabitants	0	0	0	0.01	766
Average project size, thousand euros (PPP)	363.49	1370.96	0	31706.99	766
Industrial jobs per capita	0.07	0.04	0.02	0.5	766
Illiteracy rate	0.28	0.09	0.04	0.70	766
Log of population	7.83	0.66	5.7	10.18	766
Plants per capita	0.04	0.01	0.01	0.14	766
% of population active in agriculture	0.37	0.14	0.04	0.72	766
% of households with kitchens in 1951	0.95	0.07	0.45	1	766
% of households with electricity in 1951	0.65	0.23	0	0.98	766
% of households with no water/electricity	0.01	0.03	0	0.32	766
Turnout at elections	87.28	6.01	63.1	97.10	766
Votes' share of DC (also if in coalition)	0.44	0.21	0	1	766
Votes' share of PCI	0.41	0.13	0	0.93	766
Votes' share of right wing coalitions	0.05	0.11	0	0.72	766

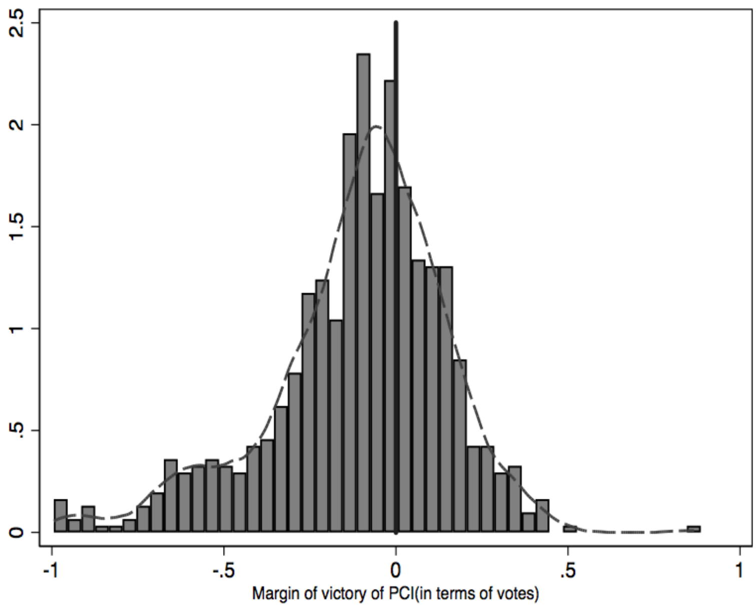
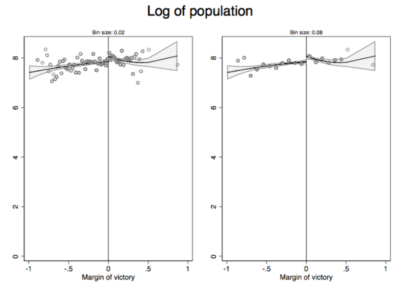
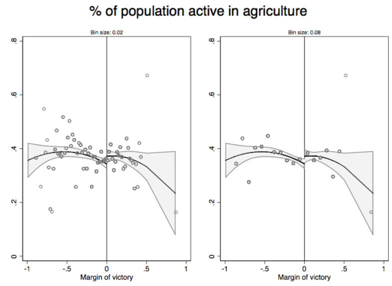


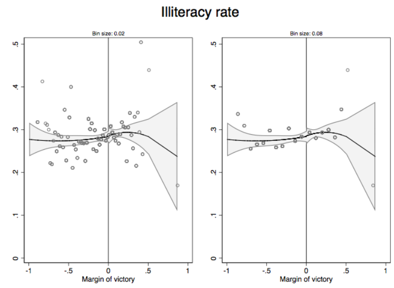
Figure 1.25: **Density distribution of the running variable, VMV for PCI**
This graph shows the density distribution of the running variable together with the plot of kernel density estimates.



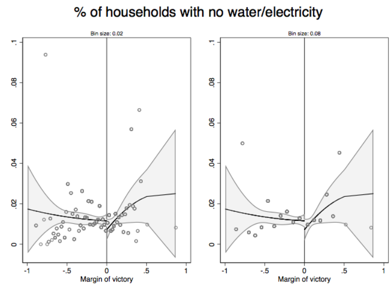
(a)



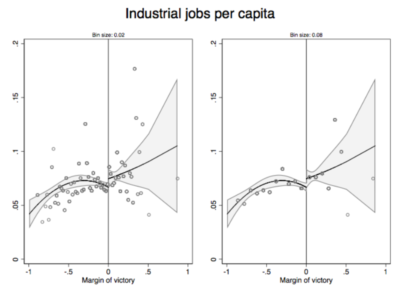
(b)



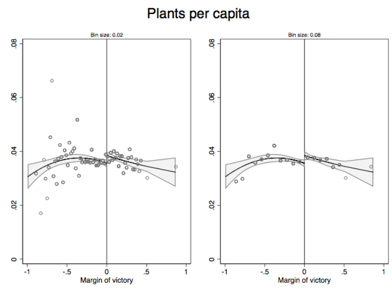
(c)



(d)



(e)



(f)

Figure 1.26: **Covariates and margin of victory of PCI.** These graphs show the average of each variable within bins of the difference in the share of votes between PCI and the main opponent party. All metrics are based on population and industry census of 1951. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

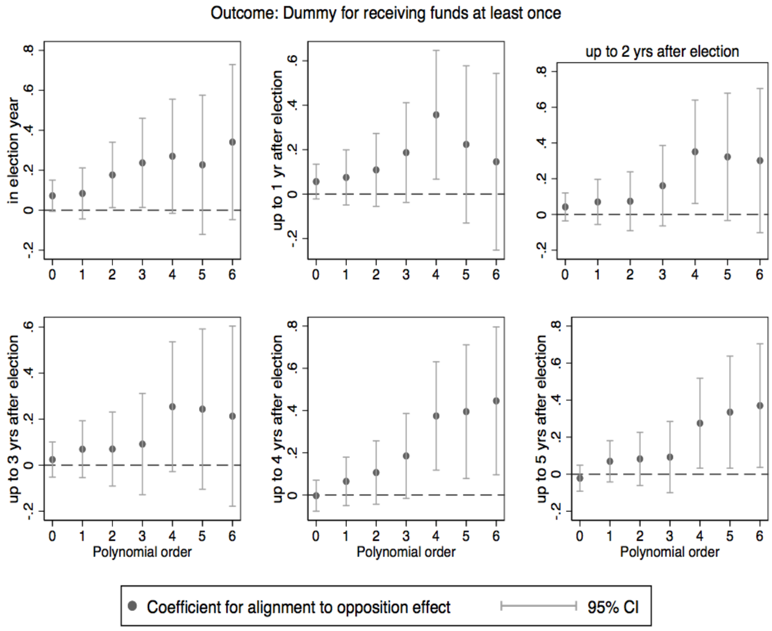


Figure 1.27: **Extensive margin - PCI races. Coefficients from parametric regressions.** These graphs plot the coefficients of a dummy for having received at least one grant, defined in different time periods after the elections, against polynomial orders of the running variable, that is VMV for PCI. The coefficients come from 2SLS regressions where the assignment to the treatment is the instrument for PCI winning the elections. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality.

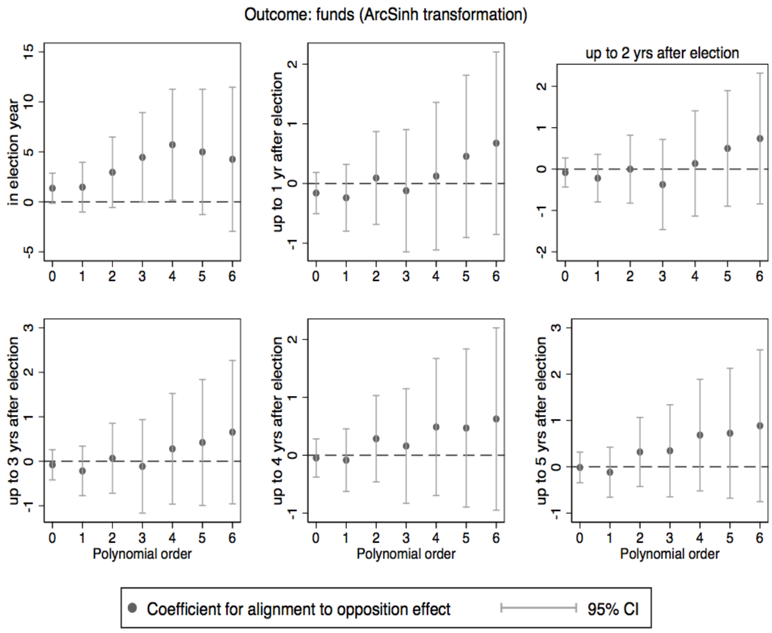


Figure 1.28: **Intensive margin - PCI races. Coefficients from parametric regressions.** These graphs plot the coefficients of the money allocated (Arcsinh), measured in different time periods after the elections, against polynomial orders of the running variable, that is VMV for PCI. The sample is restricted to the municipalities that received at least one grant within one year from the election. The coefficients come from 2SLS regressions where the assignment to the treatment is the instrument for PCI winning the elections. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a municipality.

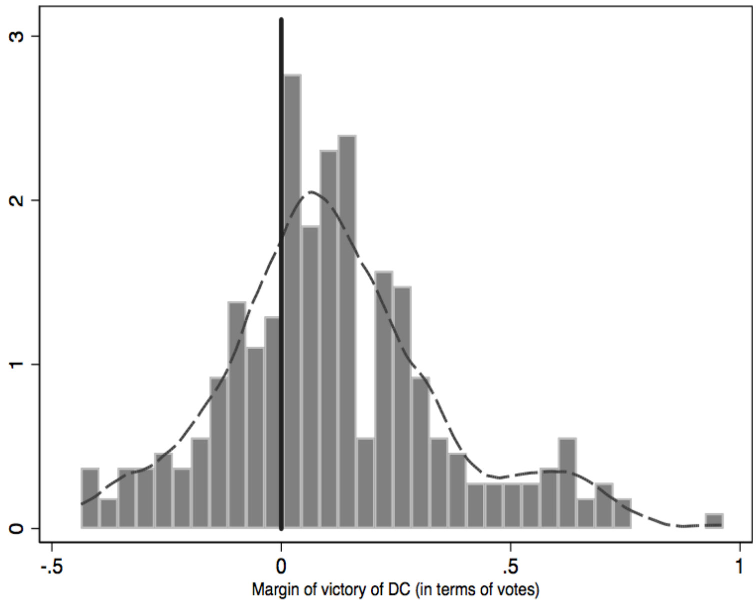
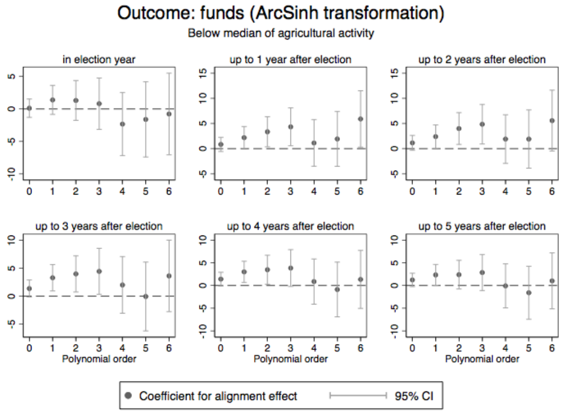
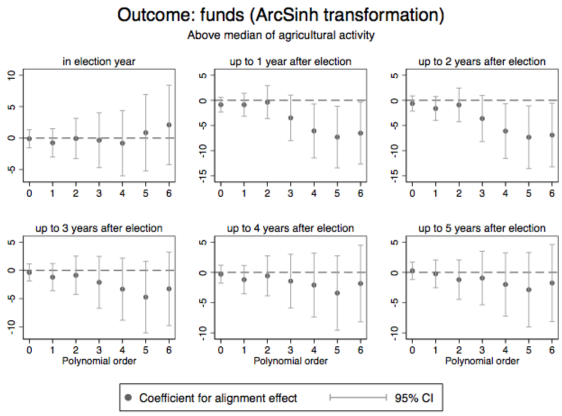


Figure 1.29: **Density distribution of the running variable - DC vs PCI races** This graph shows the density distribution of the running variable together with the plot of kernel density estimates. The sample is restricted to races of DC vs PCI only.

1.B.4 Heterogeneity



(a)



(b)

Figure 1.30: **Parametric regressions in subsamples split by below and above the median of agricultural activity.** These graphs show the coefficients and confidence intervals from the parametric regression by different orders of polynomials. Agricultural activity is measured as the share of resident population active in agriculture in 1951.

Table 1.17: Heterogeneity analysis III: population density and turnout

	Arcsinh of money allocated by years after election											
	Panel A: Population density											
	Below median						Above median					
Up to x years after election	0	1	2	3	4	5	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DC wins (Seats)	0.873	-0.605	-1.513	-0.143	-2.196	-0.790	-0.743	-0.496	-0.0947	1.239	2.062	1.212
	(2.373)	(2.416)	(2.568)	(2.346)	(2.269)	(2.121)	(2.170)	(2.232)	(2.225)	(2.330)	(2.238)	(2.337)
Observations	176	186	162	186	168	190	210	204	210	210	206	204
BW size	0.169	0.176	0.154	0.179	0.161	0.182	0.167	0.160	0.164	0.167	0.162	0.160
	Panel B: Turnout at elections											
	Below median						Above median					
Up to x years after election	0	1	2	3	4	5	0	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DC wins (Seats)	1.657	-1.080	-0.464	-1.159	0.726	0.605	-0.388	0.707	1.307	3.115	2.070	1.269
	(2.398)	(2.449)	(2.122)	(2.319)	(2.141)	(2.436)	(2.317)	(2.063)	(2.373)	(2.456)	(2.451)	(2.527)
Observations	176	176	186	176	184	160	188	240	200	204	198	186
BW size	0.228	0.233	0.267	0.227	0.259	0.197	0.123	0.173	0.131	0.134	0.129	0.122

Notes: the table reports estimates of the treatment effect from a local linear regression. The dependent variable is the inverse hyperbolic sine transformation of the cumulated money defined in different time periods after the elections. Each panel shows the results according to different splits of municipality characteristics. The bandwidth is the optimal MSE-minimising bandwidth.The unit of observation is a municipality. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

1.C Historical sources

1.C.1 IBRD Report

"Prior to the elections of 1946, the people of southern Italy and the islands were strong monarchists and politically showed majority in the Christian Democrat Party. The voting in the election of 1946 on the issue of creating a republic showed a large majority in favor of maintaining the monarchy. The left-wing parties at this time were able to show only a small minority vote. Following the defeat on the issue of the republic in 1946, however, and as a result of an intensive campaign by the communist Party (based largely on promises of a distribution of land to small farmers by breaking up large estates), a significant shift in large areas of the South and on the islands to sympathy with the Communist Party occurred during the interval between 1946 and 1948 elections. An aggressive campaign for land reform a other means of' obtaining increased employment was started which the Government could not ignore and on which, in fact, it was required to take some action. The need for action was, in fact, forced upon the Government as a result of forceful confiscation of some lands in the South of Italy by members of the Communist Party following the 1948 elections. While the development of the south is an old issue in Italy, it is against this background that the current development program for South Italy was authorized in an effort to counter the plans proposed by the communists and to increase, if possible, the political prestige of the Government in these areas."

1.C.2 Communist Party's view

This section presents parts of the speeches given by two MPs of PCI during the parliamentary discussions about CasMez on 18/11/1948 (Pellè, 2009).

Giorgio Amendola

"We want first of all to be against the creation of a "special agency, with its own legal personality, called "Cassa per il Mezzogiorno". The reasons behind this are not only related to the same reasons why many scholars, groups, associations and even some majority MPs are critical about the idea of subtracting such a large share of public expenditures from the the ordinary control of the State and from effective ex-ante and ex-post audit of the Parliament. The arguments the majority made in favour of this have not convinced us. They say: simplification, practicality, speed [...]. They claim that the state apparatus is "bulky" but then they create one more apparatus. Is this *special* bureaucracy better than the ordinary one? We doubt it. [...] The new Cassa should have a bureaucracy similar to the one of IRI (an Italian public holding company making public infrastructure investments, a/n), by law under state control but de facto prone to be controlled by few financial groups[...]. There is also the issue of parliamentary control. We cannot take away from the Parliament the right to precisely control the expenditures of public money, that can sum up to 100 billions per year. They tell us: you will be able to know the plans as they will be announced at the beginning of the year. Article 1 of the draft bill mentions the design of a general programme by the Committee of ministries; but the actual specific programs will be prepared, coordinated and executed by the Casa per il Mezzogiorno with its legal personality and, thus, autonomy. And these specific programmes are the actually relevant ones, that we want to be able to know, discuss and approve [...]. Beyond all these arguments, we have a more general and substantial criticism. Which will be the action that Cassa will be able to take on, given the situation of the South. It will be an agency with wide financial and executive powers, that will be able to realise important financial operations, discount taxes, discount interest rates, to issue bonds, to obtain loans from banks or institutions abroad, to buy shares in private companies. All these opportunities, all these powers make Cassa a very important centre. The most important for the southern economy. A centre of Italian and foreigners' financial interests, that will naturally shape Cassa's activities. It will become a centre of influences and corruption, out of any control, and meant to have relevant function in southern Italy's life, it will be a governor of southern Italy[...]. When we look at a a public agency, like the Bank of Naples, that funds with public money (coming from taxpayers and savers in the South) political newspapers and local authorities, and nominates directors who are the main representatives of fascist journalism, through connections with monopolistic groups. such as the South company for electricity [...] **we can very well foresee what this Cassa will do to all sectors of social, economic and political life in the South. It is a powerful tool of electoral and political corruption that you want to put up, for your party's own interest, to establish the empire of your regime in the south of Italy**. "

Mario Alicata

"First of all, CasMez is obeying to America directives[..]. The point is that you want to create this particular instrument to make it the particular instrument of a particular policy. It is not by chance that we have heard that CasMez will have a big loan from the Import Export Bank, that CasMez will also benefit from private American investments and so on. It is an instrument to penetrate the American capital and influence into the South. And we reject this not only for the general reasons we have always highlighted, but also- I hope that Campilli [Minister of Finance] will confirm I am right on this!- because certain investments of American capital in the South will not get rid of the old political and social structure of the South, that you claim you plan to remove, but they will actually make it stronger. This is the truth, and this is why this American inspiration behind CasMez is a threat for the South."

1.C.3 Propaganda leaflets

(a) DC - *The red fear*: “Vote or he will be your patron”

(b) FDP - *Truman's puppets*: “All united against Truman' slaves”.

Figure 1.31: **DC vs FDP - 1948 parliamentary elections**

(a) Man 1: Great, your answer is correct. Now for 40 thousands lire tell me which is the organization or political movement that since the end of the was has been an obstacle to any effort of reconstruction. Man 2: The Italian communist party.

(b) Man 1: Great, your answer is correct. Now listen carefully. After the war for the first time in the history of our country the problem of social and economic underdevelopment of the South has been dealt with. For 1,250,000 lire can you tell me who should get the merit for this great work? Man 2: the democratic government and all its allies.

(c) Man 1: Perfect! You are very well prepared! Congratulations, mr. "small voters". Now the last question, for 5,120,000 lire. Summing up all the answers you gave us, which party does your conscience tell you to vote for at the next local elections? Man 2: For the Christian Democrats? Man 3: Perfect! You win more than 5 millions! You win freedom!

Figure 1.32: **DC's propaganda for 1956 elections.**

Chapter 2

Where exactly? Targeting place-based policies: evidence from *Cassa del Mezzogiorno*

2.1 Introduction

Place-based policies are interventions targeting specific areas with the aim of improving their economic performance. Their particular focus on a defined geographical area puts them in contrast with *person-based* transfers of resources, such as welfare programs. Governments in industrialised countries often resort to this class of policies, well-known examples of which include enterprise zones, the EU structural funds and more *ad-hoc* infrastructural interventions, like the Tennessee Valley Authority (TVA) or the Appalachian Regional Commission (Neumark and Simpson, 2015). Economists have raised concerns that these policies would generate large distortions in economic behaviour and that any local benefit may cancel out at the aggregate level (Glaeser and Gottlieb, 2008). Whether place-based policies are actually effective is then often an empirical question which a large literature has tried to answer (see, for instance, Becker, Egger, and Von Ehrlich, 2010; Busso, Gregory, and Kline, 2013; Kline and Moretti, 2014a). Even though the findings have been inconclusive, scholars seem to agree that a more informed policy debate requires an understanding of the overall welfare effects of place-based policies as well as their initial economic rationale (Kline and Moretti, 2014b; Duranton and Venables, 2018). In this paper I stress the importance of shifting the focus from the initial and theoretical rationale to those factors which are actually guiding the policy implementation in reality. This argument is motivated by a large body of research which has demonstrated the relevance of strategic or non-economic factors in the distribution of public funds ranging from fiscal policy to foreign aid (Larcinese, Rizzo, and Testa, 2006; Faye and Niehaus, 2012). Very little is known, however, whether these insights also apply to place-based policies.

In this paper, I study to which extent the allocation of a large place-based policy in post-WWII Southern Italy *within* its target area was determined by local labor demand shocks rather than the intended long-term development planning. *Cassa del Mezzogiorno* (henceforth CasMez), was adopted by the Italian government in 1950 to fund large infrastructural investments and improve the economic performance of the underdeveloped southern regions. Initially designed as as a 10-year development plan, this intervention lasted for 40 years and included several types of policy instruments beyond infrastructural investments, such as direct subsidies or relocation incentives for firms. I quantify the money allocated by CasMez to each municipality in Southern Italy and identify how local labor demand shocks affected changes in the allocation of CasMez money. To isolate the causal effect of local economic shocks, I combine the time variation in the industry-level growth rate of jobs in Southern Italy with cross-sectional variation in the industry structure of the municipalities to instrument for the jobs growth rate at municipality level. The results show that CasMez funds were allocated in a pro-cyclical way to municipalities with faster jobs growth. This seems at odds with the original aim of CasMez to support the industrialisation of poorer areas.

After WWII, the historical regional disparities between the North and the South of Italy were expanding. GDP per capita of southern regions reached barely 55% of the one in Centre-North (Svimez, 2011). The Italian government, with the strong support of the International Bank for Reconstruction and Development (IBRD) and the US government, reacted by designing a place-based development policy, called *intervento straordinario*, the core of which was the institution of CasMez. CasMez was initially modelled as an independent agency, with technical skills to design and implement large infrastructural projects for the *pre-industrialization* of the most depressed areas in the South, such as land reclamation and settlement, aqueducts, and roads. After the first seven years of activity, the intervention was extended to also include direct subsidies to firms and incentivize *industrialization*. After several institutional changes and declared lack of success, CasMez was eventually dissolved in 1993[1].

CasMez constitutes a compelling case study for studying the allocation of a place-based policy for a variety of reasons. First, the policy was targeted at a very large area and this creates a high degree of variation in recipients' characteristics. As an example, when CasMez activity started there were on average 120 non-agricultural jobs every 1000 inhabitants in municipalities in Sardinia, but only

[1] Precisely, it was suppressed in 1984 when a new agency, AgenSud, with very similar characteristics was created to substitute it.

80 in Basilicata. Second, the policy spans an extremely long period of Italian economic history: it was created in the aftermath of a war and continued during a period of unprecedented growth, the so-called *golden age*. Its activity then went on during periods of severe international macroeconomic instability induced by 1970s' economic shocks[2]. This allows for a large degree of variation in national and local economic trends. Finally and most importantly, the socio-economic gap between Northern and Southern Italy is still very wide today. In 2017 the unemployment rate in the population with a degree was 11.1% in southern regions, against 6.4% in the country as a whole and 4.2% in the Northern regions (ISTAT, 2017b)[3]. Four of the southern regions are also part of the convergence area of the EU as their per capita gross domestic product (GDP) is less than 75% of the EU average (EUROSTAT, 2017). Any historical evidence on CasMez can thus be informative to current policy-makers on more or less successful features of public support to the development of disadvantaged areas.

Even if CasMez allocated the equivalent of 140 billion euros between 1951 and 1993 to southern regions, the actual distribution of funds at the micro level is largely unknown. To fill this gap, I use the dataset on the universe of projects funded by CasMez, provided by the Ministry for Economic Development. I then construct a novel municipality-level dataset, which is linked to decennial population and industry censuses for the four decades following the introduction of CasMez in 1950. To measure local growth rates, I use the percentage change in jobs per capita in each municipality. Identifying the effect of local economic growth rates on the allocation of funds is challenging because public investments can be themselves a determinant of local growth. I tackle this issue by constructing a shift-share instrument, in which the pre-CasMez industry structure of municipalities is used to predict the impact of Southern Italy's aggregate industry-level growth rate of jobs

[2]In numbers, GDP growth went from -10.27% in 1945 to 8,41% in 1950 to 1.61% in 1971 to go up again in the mid-80s and collapse to 1% at the beginning of the 90s (Baffigi, 2011).

[3]Beyond economic development, there are severe disparities in other important aspects, such as access to health and education services. For instance, while public nurseries in the northern regions can offer on average 18 slots every 100 inhabitants under 2, in the South this number goes down to 4 (ISTAT, 2017a).

per capita on the changes in CasMez funds allocation at the local level.

My baseline results show that, between 1951 and 1991, funds grew faster in municipalities with higher decennial growth rates of jobs per capita. In terms of magnitude, a 1 percent increase in jobs per capita growth rate is associated with roughly 1 percent increase in the growth rate of funds. In particular, comparing OLS and IV coefficients suggests that OLS estimation understates the true effect of decennial employment growth rate on the growth of CasMez funds at the municipality level. I argue that potential determinants of this downward bias are measurement error or the presence of clientelistic networks. These results are robust to several specifications such as decade fixed effects, decade-specific effects of pre-CasMez controls and the inclusion of municipality linear trends as well as non-linear trends at higher levels of aggregation.

I then exploits some dimensions of heterogeneity to shed further light on the mechanisms behind the baseline results. First, I look at cross-sectional differences in pre-CasMez levels of development and find that the response of CasMez funds to job growth was larger in municipalities with higher initial *levels* of both industrial and social development. More precisely, the results are predominantly driven by municipalities in the top quintile of the distribution of pre-CasMez jobs per capita. Secondly, I replicate the main analysis by using different measures of the intervention. I find that the growth in the flow of money is coming from CasMez funding larger rather than more projects to growing municipalities. I also find that the results are very similar when outcomes are measured as levels and not as growth rates.

Overall, these results suggest that the allocation of CasMez was influenced by local business cycles. In particular, the largest investments were targeted at areas that were growing faster and had better growth prospects. These findings seem at odds with the official intentions of CasMez to create the basis for self-sustained growth in less developed areas (Podbielski, 1978) and suggest that other allocation criteria might also have been at work. One interpretation is that, as this policy lost very early its temporary nature, it might have become more ordinary by responding to economic shocks over time. Also, vote-maximising behaviour might have incentivised policy makers to divert larger and riskier investments towards more promising areas to increase support for the central government. This view can also be interpreted consistently with political agency models that predict procyclical fiscal policies in developing countries where voters do not trust governments (Alesina, Campante, and Tabellini, 2008). Beyond these motives, the findings can be consistent with other rationales for place-based policies highlighted in the literature. First, subsidizing unproductive places is an imperfect way of transferring resources to more depressed areas because of mobility responses (Kline and Moretti, 2014b). Secondly, spatially concentrated funding can be used to exploit agglomeration economies. Also, these results could point to a demand-based allocation mechanism, if growing areas were also more likely to ask for funds, especially for direct industrial subsidies (Felice and Lepore, 2017).

This paper is linked to various strands of the economics literature. First, I add to the existing line of empirical research on place-based policies[4]. Within this area, my work is closest to Becker, Egger, and von Ehrlich (2012) for their focus on the distribution of funds. The authors find that transfers of EU regional policy generate faster growth in recipient regions but argue that overall a different distribution of funds would have led to higher aggregate efficiency and growth.

My analysis also speaks to the large body of research on additional motives of public transfers. On the one hand, this paper is connected to the literature on the strategic determinants of government's transfers within countries, as in Burgess et al. (2015), Bracco et al. (2015) or Carozzi and Repetto (2016). On the other hand, the nature of CasMez as a development agency puts this work close to studies on the determinants of development aid that analyse the impact of strategic or political factors unrelated to the need of recipient countries (see Faye and Niehaus, 2012; Kuziemko and Werker, 2006; Eisensee and Strömberg, 2007). There are, however, fewer studies focussing on how aid is subsequently distributed *within* the recipient country, as noted by Qian (2015). One of the few exceptions is Jayne et al. (2002),

[4]See Neumark and Simpson (2015) for a comprehensive review.

who find evidence for spatial rigidity of food aid allocations in Ethiopia and explain it with inertia in the allocation mechanisms. My work builds on both literatures but shifts the attention to the role of local economic cycles.

Finally, this paper relates to a growing literature in economic history on CasMez. Most of this research focuses on qualitative or aggregate appraisals of CasMez intervention and finds that, while CasMez contributed to the convergence of southern regions in the 1950s, institutional changes and corruption were responsible for the later ineffectiveness of the policy, especially after the mid-1960s (see in particular Felice and Lepore, 2017; Papagni et al., 2018). All these works are contributing to improve the knowledge about the evolution of CasMez over the years of its activity. However, the unit of analysis is usually the region; while this can give important insights on macro trends, it does not furnish a valid base for rigorous econometric evaluations. To my knowledge, this work provides the very first comprehensive quantitative analysis of the economic criteria guiding the allocation of CasMez at the micro-level for over 40 years.

The rest of the paper is organized as follows. Section 2.2 outlines the economic conditions of Italian southern regions after WWII and the birth and development of *Cassa del Mezzogiorno*. Section 2.3 describes the data sources as well as the construction of the variables of interest. Section 2.4 presents the methodology and discusses the identification strategy. Section 2.5 describes the results from the baseline specification and the heterogeneity analysis. Section 2.6 concludes.

2.2 Background: *the southern question* and CasMez

WWII had a devastating impact on the Italian economy Italy with GDP in 1945 being at the same level as of 1906 (Toniolo, 2013). Damages to infrastructures from both Nazi Germany's occupation and Allies' bombing have been estimated to reach at about 52 million dollars (Pellè, 2009). Universal access to basic amenities was not guaranteed and half of the population was undernourished during the years 1946-47 (Vecchi, 2011). However, similar to many other Western European countries, Italy soon entered a period of high economic growth, the so-called *golden age*. GDP per capita increased by 85% from 1945 and 1949 alone (Felice, 2015). The literature generally agrees in defining the post-war settlement and the European Recovery Program as key ingredients for this rapid growth. Recently, Giorcelli (2016) has shown that, thanks to US grants, the industrial production in Italy increased by 20 percent and reached the pre-war level by 1950 already.

However, the historical North-South divide did not improve after the war[5]. Even if not all scholars agree on the timing and determinants of the persisting regional differences in Italy, commonly referred to as the *southern question*, it is widely accepted that both world wars sharply increased the economic gap between North and Southern Italy (Felice and Lepore, 2017). By the end of the 1940s, northern regions were experiencing high growth rates while the South was still facing very low level of industrialisation with 34% of GDP coming from agriculture compared to 19% in the Centre-North (Podbielski, 1978). This was accompanied by stark differences in living conditions: while 58.9% of inhabitants in the South had no or very low consumption of meat, sugar and wine, this was only 6.9 % in northern regions (Vecchi, 2011).

This situation as well as strategic political considerations[6] were the driving factors that pushed the Christian-Democrat government, which was strongly supported by the US government and the IBRD, to launch an economic intervention called *intervento straordinario*, at whose core was the creation of *Cassa del Mezzogiorno*. CasMez was a state-owned agency, initially designed to last for 10 years in order to plan and implement basic infrastructural projects in the deprived areas of the South. Identifying a common rationale of CasMez intervention over the entire 40 years is challenging, especially because the initial mandate was extended and modified multiple times. Scholars have generally agreed that the history of CasMez can be divided into four main phases (Felice and Lepore, 2017):

[5] Eckaus (1961) estimated a 15 to 25 percent differential in income per capita between the North and the South already in 1861, the year of Italy's unification.

[6] Southern regions expressed a clear preference for monarchy during the 1946 referendum, after which Italy became a Republic. Moreover, peasants were strongly opposing the central government through widespread occupations of lands (Barucci, 1978).

- 1950-1957: *pre-industrialization.* The very first plan of CasMez included projects to provide deprived areas with basic infrastructures and create a *big-push* towards self-sustained growth. Projects were mainly related to reclamation, transformation and settlement of lands, aqueducts and severs, roads and, to a smaller extent, tourism.

- 1958-1970: *industrialization.* CasMez was significantly refunded and started to directly subsidise industries and target clusters to stimulate the emergence of agglomeration economies with the explicit goal of creating more jobs.

- 1970-1984: *regions.* The implementation of the policy changed drastically as the newly born regions entered the decisional and planning process. Increasingly *ad-hoc* measures were taken and room for tactical distribution increased.

- 1985-1993: *phase-out.* CasMez is suspended in 1984 and a new agency, AgenSud, is created acknowledging that the intended results had not been achieved. AgenSud was substantially different from CasMez and can be seen more like a formal institution to gradually phase-out State's direct intervention in southern regions.

In terms of operative structure, CasMez was designed to mimic the experience of the Tennessee Valley Authority (TVA). In practice, however, it was quite different from its prototype. The TVA had full powers to solve one uniform technical issue, without having to report to central structures (Svimez, 2015). CasMez instead was only responsible for the technical planning of the projects and the supervision of their implementation. The detailed administration and execution of the projects was in the hands of appointed existing agencies, usually provincial or regional departments. Moreover, while the general technical planning was carried out by CasMez, it had to meet the development strategies which were designed at parliamentary or government level. The lack of clear government directives and the need to make fast decisions were often calling for more standardised and less sophisticated planning and intervention. CasMez's own director (Pescatore, 1961) noticed already in the 1960s that the government committee responsible for determining the general scope of the policies often ended up formulating policies on an *ad hoc* basis.

Evaluating the overall welfare effects of 40-years of CasMez on southern regions' is extremely challenging. Scholars, however, tend to agree that the first 15 years of activity were strongly correlated with the very first and last episode of convergence by southern regions towards those in the North (Daniele and Malanima, 2007; Felice and Lepore, 2017). In the first two decades of CasMez activity, the South's GDP per capita rose at an average annual rate of 5.77 percent (Iuzzolino, Pellegrini, and Viesti, 2013). Yet, scholars also agree that some institutional and economic changes have contributed to the decrease in the ability of CasMez to stimulate the economy of southern regions (d'Adda and de Blasio, 2017). Sociologists have highlighted that two crucial unintended consequences of this large state intervention were corruption and the creation of a deep economic dependency of southern regions on state aid (Trigilia, 1992). Some scholars have even argued that CasMez might have contributed to the increasing divergence *within* the South itself (Chubb, 1982).

2.3 Data

The quantitative analysis of this paper is based on three main data sources: a project-level dataset on CasMez and Industry and Population Censuses that cover the 1951-1991 period. The CasMez project-level dataset was provided by the Ministry of Economic Development and includes information on timing, location, size and type of the universe of projects funded by CasMez. In order to merge this information with the two types of census data, I created the very first municipality-level dataset for CasMez, by allocating projects to time comparable units of observation and taking into account all border changes which occurred between 1951 and today. While almost 90% of the projects can be matched to a single municipality in the Ministry's database, 48% of the total funds are coded as being targeted to public works projects referred to as *pluricomunali*, i.e. *multi-municipality.* For those *multi-*

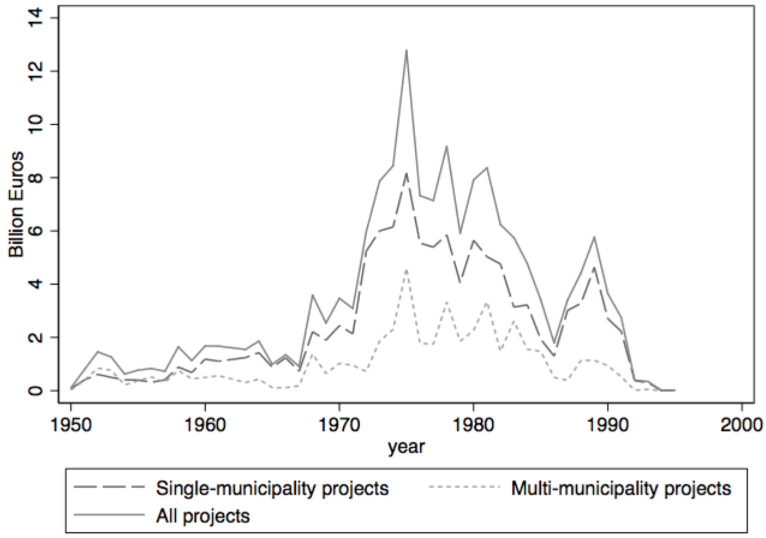


Figure 2.1: **CasMez funds**

municipality projects that are recorded without a list of targeted locations, I have extracted location names and other geographical references from the project title[7]. For each municipality, I can thus reconstruct the number of projects and the corresponding flow of money received over a decade. All monetary values are converted to their 2011 value, through the conversion coefficients for historical comparisons provided by the Italian Statistical Office (ISTAT).

The data from the decennial industry and population censuses from 1951 to 1991 have been obtained from different electronic and paper sources[8]. From these datasets I create the main independent variable as well as a large set of municipality-level controls. The population census provides not only the count of residents but also other demographics, such as age, literacy level, household conditions and residents active in agriculture. The industry census has a wide coverage of economic

[7]More details on how I implement this process, how I track border changes and on the data cleaning process in general can be found in appendix 2.A

[8]I have digitized the population census for 1951 and 1961. The other waves from the population census have been extracted by Atlante Statistico dei Comuni, available for download from ISTAT website. The industry census data is also available in electronic format on ISTAT website. The comparison between the different waves of the industry census can be done thanks to the reconstructed time series provided by ISTAT, that take into account all the differences in data collection that occurred over the years. For this reason, I do not use data before 1951. Moreover, because of the war, no census was carried out in 1941. For more details and all definitions used see Istat (1998).

activity and provides the number of employees in each plant by sector in each municipality. I use 3-digit sectors to allow for meaningful comparisons, as for some economic activities a higher level of disaggregation was introduced in 1961 only. Appendix 2.B reports the list of industries of the final dataset. To allow comparisons from 1951, the following industries are not included in the analysis: fishing, legal services, informatics, R&D, public administration, education and health services.

In order to measure local labor demand shocks, I look at the percentage change of employment rate (ER). However, since municipality-level data on employment rates were collected by the Italian Statistical Office (ISTAT) only from 1977 onwards, I proxy ER with the *number of jobs per capita*, henceforth JPC. JPC is constructed by dividing the number of employees by the resident population older than 10 years[9]. The main difference between JPC and ER is that the former metric approximates the number of jobs in a given location while the latter measures the number of residents with jobs. This implies that the metric I use has three main limitations:

1. For JPC the statistical unit is a plant or activity, while for ER it is a person. JPC cannot easily detect people that work from home as well as free lance or independent workers.

2. JPC is measured at the location where the activity takes place while ER is measured where the person lives. Especially at the municipality level, the difference between JPC and ER can be large if workers are mobile.

3. JPC does not take into account participation in the labor force.

In general, JPC is likely to underestimate the level of employment rate. Yet, it might also overestimate it whenever there is a very large inflow of commuters in the municipality. In terms of growth rates, however, the difference between the two

[9]Employees are defined as any employees, dependent or independent, full or part-time, with permanent or temporary contracts.

metrics will depend on many factors[10]. Finally, a disadvantage of JPC is that, as it is derived from the industry census, it does not cover the entire economy and, in particular, it does not cover agriculture. In order to take trends in the agricultural sector into account, I include the proportion of residents active in agriculture as a control in all regressions. As this variable is derived from the *population* census it cannot be simply added to the count of jobs when creating JPC.

There are also some advantages of using JPC over ER. First, it provides a more uniform economic classification as it is based on the economic activity declared by the plant owner while ER is based on individual declarations. Secondly, JPC is based on a spatial statistical unit defined in terms of economic activity which may actually serve a better proxy for the target of a place-based policy. In the industry censuses these local units are defined as *unità locali* and are all the plants or groups of plants where the firm materially carries out its activities to produce goods or services.

These three data sources have been linked together to create a panel of 2,810 municipalities over 4 decades and 5 points in time. Figure 2.2 shows the area studied in the analysis. The data covers all the southern regions and Lazio where half of the municipalities were targeted by CasMez[11]. Table 2.1 reports summary statistics for the main variables used in the analysis. As expected, JPC growth and the distribution of funds exhibit very large variation over four decades.

[10]We can write the growth rates of JPC and ER as:

$$GrowthRate(JPC_{id}) = GrowthRate(\frac{\#ofjobs_{id}}{pop_{id}^{>10}}) = GrowthRate(\#ofjobs_{id}) - GrowthRate(pop_{id}^{>10})$$

$$GrowthRate(ER_{id}) = GrowthRate(\frac{\#ofemployed_{id}}{pop_{id}^{15-64}})$$

which is then equal to

$$GrowthRate(\#ofemployed_{id}) - GrowthRate(pop_{id}^{15-64})$$

Assuming that the working age population is growing at the same rate as the population over 10, the $GrowthRate(JPC_{id})$ will be overestimating $GrowthRate(ER_{id})$ whenever jobs in a municipality are growing at a faster rate than people with jobs.

[11]Marche, Toscana and Umbria are excluded from the dataset because only a very small area was targeted by CasMez. In each region less than 4% of all the municipalities received money at least once. Results are also robust if Lazio is excluded from the sample.

Figure 2.2: **CasMez: target area**
Provinces targeted by CasMez and included in the sample are highlighted in grey.

Table 2.1: Descriptives

Variable	Mean	Std. Dev.	Min.	Max.	N
Funds-million euros	10.9	58.58	0	2697.72	14050
Funds per capita-euros	1982.58	8986.46	0	578250.75	14050
10-year growth of funds	0.36	1.39	-6.33	5.87	11240
# of projects	12.59	34.57	0	1187	14050
Average funds per project-million euros	0.55	2.43	0	161.95	14050
Jobs p.c. (JPC)	0.1	0.08	0.01	3.12	14050
JPC growth	0.24	0.66	-0.91	19.47	11240
Plants p.c. (PPC)	0.05	0.01	0	0.23	14050
Area in sq km	49.91	67.05	0.12	1501.25	14050
Dummy for coastal municipality	0.16	0.37	0	1	14050
Illiteracy rate 1951	0.27	0.09	0.03	0.70	14050
% of households without water in 1951	0.47	0.31	0	2.35	14050
Log of population in 1951	7.99	0.94	5.38	14.15	14050
% of population active in agriculture in 1951	0.37	0.14	0.02	0.84	14050
Population density in 1951	167.73	352.56	6.95	8084.58	14050

Notes: the sample is made of 2,810 municipalities observed at 5 points in time, over four decades. It shrinks to 4 points in time when computing growth rates. For per capita measures and shares, the denominator is the population older than 10.

2.4 Methodology

To investigate the relationship between the allocation of CasMez funds and local economic growth, I estimate the coefficient β in the following equation:

$$\Delta funds_{id} = \beta \Delta jpc_{id} + (X_{i0} * \eta_d)\delta + \eta_d + \psi_i + \epsilon_{id} \tag{2.1}$$

where variables in lower case indicate logarithmic transformations of upper case level variables so that $\Delta funds_{id}$ represents the growth rate of CasMez funds in municipality i between decade *d-1* and d and Δjpc_{id} denotes the growth rate of the level of jobs per capita JPC. Decade fixed effects, η_d, are included to absorb the impact of aggregate economic shocks. I also include a vector of interactions between pre-intervention covariates, X_{i0}, and decade fixed effects which controls for the time-varying impact of initial conditions on both JPC and funds growth rates. X_{i0} comprises of geographical factors (altitude, dummy for coastal locations and log of the municipality area in square kilometres) as well as socio-economic determinants (population density, log of population, percentage of population active in agriculture, illiteracy rate and percentage of households with no access to water). ψ_i are municipality fixed effects controlling for trends in the outcome and independent variable. The standard errors in all regressions are clustered at the municipality levels, but results are robust to clustering at province and commuting zones level too.

This specification may still not account for several sources of unobserved heterogeneity which could lead to a bias in the OLS estimate of β, the direction of which is ambiguous ex-ante. Two major concerns are simultaneity and reverse causality. Local economic growth is presumably determined by the changes in CasMez funds over time and the estimation of (2.1) cannot isolate this contemporaneous effect from the response of CasMez to local shocks. More generally, any constant and time-varying unobservables could co-determine local economic growth and CasMez allocation. One example of this would be the presence of clientelistic networks. These may cluster in thriving areas where they are able to capture more of the policy's funds and are also more likely to influence the economic development. Finally, there can be some error in the measurement of Δjpc_{id} [12] which if correlated with the error term maylead to attenuation bias.

For all these reasons, I employ an instrumental-variables (IV) strategy to account for the potential endogeneity in the growth rate of JPC. Following Bartik (1991), I predict the growth rate of JPC with a weighted average of the aggregate industry-level growth rates observed in the South[13]. The idea of this instrument is to combine the exogenous time variation deriving from the aggregate industry-level growth rate with the pre-determined cross-sectional variation deriving from initial industry-shares which determines the degree of *exposure* to the time variation. Δjpc_{id} is then proxied by the growth rate one would observe had each municipality grown at the economy-wide growth rate and had the industry composition not changed. The used weights are the shares of JPC in each 3-digit industry in a given municipality at the beginning of the sample period in 1951.

[12]See section 2.3

[13]Commonly, scholars use the national growth rate. However, this does not seem appropriate in this context as the economic structure of the North was significantly different, as mentioned in section 2.2.

In the baseline specification, I estimate the first stage as follows:

$$\Delta jpc_{id} = \gamma B_{id} + (X_{i0} * \eta_d)\widetilde{\delta} + \widetilde{\eta}_d + \widetilde{\psi}_i + \widetilde{\epsilon}_{id} \tag{2.2}$$

where B_{id} is the instrument and is equal to $\sum_s w_{is0} * \Delta jpc^{South}_{sd})$. The weights for each municipality i and sector s, w_{is0}, are defined as $\frac{JPC_{is0}}{JPC_{i0}}$. As in equation (2.1), $\widetilde{\eta}_d$ are decade fixed effects, $X_{i0} * \eta_d$ are the interactions between them and a vector of controls measured at the beginning of the sample period and ψ_i are municipality fixed effects.

By using the predicted values of Δjpc_{id} equation (2.1) can then be estimated via 2SLS as :

$$\Delta funds_{id} = \beta \widehat{\Delta jpc_{id}} + (X_{i0} * \eta_d)\delta + \psi_i + \eta_d + \epsilon_{id} \tag{2.3}$$

where $\widehat{\Delta jpc_{id}}$ is the predicted growth rate of number of jobs per capita in each municipality.

The identification of β in equation (2.3) relies on the interaction between the weights and the economy-wide industry trends to be exogenous *conditional* on observables. Arguably, the weights that measure the industrial structure are *predetermined* and this can rule out the simultaneity channel of endogeneity. However, the initial industrial structure of a municipality could still be determining the allocation of CasMez, through other channels that are not directly related to JPC growth rate. The controls I use are in fact crucial as any aspect of economic development might be an important omitted variable. Most importantly, less industrially developed municipalities are also more likely to be predominantly based on agricultural production and, as the aim of CasMez was initially to industrialise rural areas, this would constitute a channel through which the instrument may directly affect the allocation of CasMez. This makes it particularly important to control for the share of residents active in agriculture. In addition to this, I also present regressions weighted by population in 1951.

Another way to shut down the impact of unobservable shocks is to allow for non-linear trends at slightly higher levels of aggregation such as provinces and *labor market area.* or commuting zones (CZ). The sample contains 46 provinces which are administrative clusters of municipalities. Up to the introduction of regions in 1970 they were the only administrative body above the municipality. Commuting zones are constructed by ISTAT and defined as sub-regional geographical areas where the bulk of the labor force lives and works. Boundaries were defined in 1981[14] for the first time on the basis of commuters who cross the boundary on the way to work. Overall my sample contains 353 commuting zones.

As the distribution of CasMez funds is very skewed to the right and 25% of the observations are zeroes, the growth rate of the outcome variable is proxied by the first difference of the inverse hyperbolic sine transformation of the funds[15]. As suggested in Burbidge, Magee, and Robb (1988), this transformation can be preferred to a logarithmic one as it reduces the influence of extreme values while being defined for zero values of the outcome variable[16]. Results are robust to a more standard $log(1 + y)$ transformation too.

2.5 The effect of jobs growth on CasMez allocation

2.5.1 Baseline results

This section highlights and discusses the baseline results. Table 2.2 shows the estimated γ of the first stage equation, equation (2.2), across different specifications. As expected, the coefficient is positive and statistically significant in the baseline specification. The coefficient is also robust to the inclusion of controls, alone and interacted with time fixed effects, even if it drops in size. This confirms how crucial initial conditions of municipalities and their time-varying effects are as they are also correlated with the instrument. Finally, weighting of observations with population in 1951 pushes the coefficient up a bit which suggests that the growth rate of larger

[14]The boundaries are clearly endogenous as they were determined on the basis of industrial structures probably affected by CasMez and for this reason the labor market areas have not been used as unit of observation.

[15]This is $log(y + (y^2 + 1)^{(1/2)})$.

[16]Except for very small values, the inverse sine is approximately equal to $log(2y)$.

municipalities is better predicted by aggregate industry growth rates. Overall, these results rule out any kind of weak instruments concern.

Table 2.3 compares the estimates of β coefficients in the OLS and 2SLS specifications (equation (2.1) and (2.3), respectively). The odd columns report OLS coefficients while the even columns the 2SLS ones. OLS estimates are positive and significant and point to a strong correlation between the percentage changes in CasMez funds and JPC growth even after controlling for constant and time-varying effects of socio-economic development and geographical characteristics, alone or interacted with decade dummies. The IV estimates are also positive and significant but consistently larger than the OLS ones. The difference becomes smaller once the interactions of controls with time fixed effects are included. The IV coefficient drops in column 2 which indicates that the controls are capturing at least some of the variation in the changes of CasMez allocation. The relative size of municipalities is influencing the coefficient as it goes up in column 8. Based on this specification, one percentage increase in the growth rate of JPC is associated with a 1.2 percentage increase in the growth rate of CasMez funds. The coefficient is also robust to unit linear trends through the inclusion of municipality fixed effects as well as non-linear trends using interactions of region, province and commuting zone fixed dummies with decade fixed effects (see table 2.9 in the appendix). This is particularly insightful as it allows to ruling out that the detected effect is coming from trends in groups of municipalities or specific areas.

As mentioned in section 2.4, there are several factors that could bias the OLS estimation of β. To the extent that the instrument is valid, the results show that the OLS estimates are downward biased. One reason could be attenuation bias arising from measurement error as described in section 2.4. Another reason could be for instance the presence of clientelistic networks that would attract CasMez funds but hinder local economic growth.

On the other hand, IV approach measures a local average treatment effect (LATE) on compliers. Therefore, the IV coefficient could be larger than the OLS one because it is capturing the effect coming from growing areas that are more rep-

Table 2.2: First stage
JPC growth

	(1) Δjpc	(2) Δjpc	(3) Δjpc	(4) Δjpc	(5) Δjpc
Bartik IV	0.944***	0.642***	0.659***	0.572***	0.699***
	(0.071)	(0.083)	(0.086)	(0.091)	(0.111)
Time FE	NO	YES	YES	YES	YES
Controls	NO	NO	YES	YES	YES
Time FE*Controls	NO	NO	NO	YES	YES
Population weights	NO	NO	NO	NO	YES
Observations	11240	11240	11240	11240	11240
R-squared	0.0150	0.0203	0.0260	0.0360	0.115
Municipalities	2810	2810	2810	2810	2810
IV F-stat	175.3	59.12	58.25	39.59	39.49

Notes: The table reports the estimate of γ in 2.2. The instrument is the weighted average of growth of jobs per capita in 3-digits in the whole South. The controls included are measured at the beginning of the sample period and are: share of resident population that is illiterate, share of households with no access to water, log of resident population, log of municipality area in squared km, population density, share of resident population active in agriculture, average altitude in metres, a dummy for coastal areas. The units of observation are municipalities in 5 points in time. Standard errors clustered at municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.3: Baseline results: the effect of Δjpc
Outcome variable: growth rate of CasMez funds

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Δjpc	0.234***	0.860***	0.229***	0.588***	0.400***	0.950***	0.387***	1.179*
	(0.028)	(0.208)	(0.027)	(0.218)	(0.046)	(0.292)	(0.062)	(0.629)
Time FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Time FE*Controls	NO	NO	YES	YES	YES	YES	YES	YES
Population weights	NO	NO	NO	NO	YES	YES	YES	YES
Municipality trends	NO	NO	NO	NO	NO	NO	YES	YES
Observations	11240	11240	11240	11240	11240	11240	11240	11240
R-squared	0.356	0.271	0.394	0.366	0.541	0.520	0.585	0.497
Municipalities	2810	2810	2810	2810	2810	2810	2810	2810
IV F-stat		58.25		39.59		39.49		21.41

Notes: The table reports results from OLS and IV regressions. Odd columns report coefficients from OLS regression; even columns report coefficients from 2SLS regressions. For 2SLS regressions the instrument is the weighted average of growth of jobs per capita 3-digits sectors in the whole South. Kleibergen-Paap F-statistic is reported from the first stage. The controls included are measured at the beginning of the sample period and are: share of resident population that is illiterate, share of households with no access to water, log of resident population, log of municipality area in squared km, population density, share of resident population active in agriculture, average altitude in metres, a dummy for coastal areas. The units of observation are municipalities in 5 points in time. Standard errors clustered at municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.4: Reduced form
Outcome variable: growth rate of CasMez funds

	(1)	(2)	(3)	(4)	(5)
	$\Delta funds$	$\Delta funds$	$\Delta funds$	$\Delta funds$	$\Delta funds$
Bartik IV for JPC	0.592***	0.566***	0.337***	0.664***	0.852
	(0.139)	(0.133)	(0.124)	(0.222)	(0.592)
Time FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Time FE*Controls	NO	NO	YES	YES	YES
Population weights	NO	NO	NO	YES	YES
Municipality trends	NO	NO	NO	NO	YES
Observations	11240	11240	11240	11240	11240
R-squared	0.326	0.345	0.383	0.530	0.577
Municipalities	2810	2810	2810	2810	2810

Notes: The table reports coefficients from OLS regressions of the outcome variable on the instrument. The instrument is the weighted average of growth of JPC in 3-digits sectors in the whole South. The controls included are measured at the beginning of the sample period and are: share of resident population that is illiterate, share of households with no access to water, log of resident population, log of municipality area in squared km, population density, share of resident population active in agriculture, average altitude in metres, a dummy for coastal areas. The units of observation are municipalities in 5 points in time. Standard errors clustered at municipality level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

resentative of the economy-wide trends and are probably also more likely to attract CasMez investments. Table 2.4 reports the results from the reduced form which show that the interaction between predetermined industry structure and economy-wide growth of JPC affects the allocation of CasMez money. As for the first stage, the results are quite stable across different specification.

2.5.2 Heterogeneity analysis

Overall, the results presented in section 2.5.1 provide evidence that over the whole period of its activity CasMez has been targeting more areas with higher rates of job creation. There could be multiple rationales for this. For example, policy-makers might want to target to areas with higher returns and better growth prospects for efficiency reasons. Or they might want to tactically distribute funds in order to improve government's reputation. Or they might just base allocation on the demand for funds. This section examines whether the baseline effect identified in the previous section varies across both municipality and investment characteristic to gain further insights on the mechanisms behind the main results.

First, place-based development policies, like CasMez, predominantly aim to sustain poorer areas. Standard models with perfect market clearing would, however, suggest that creating incentives to work and live in poorer areas is inefficient (Glaeser and Gottlieb, 2008). This notwithstanding, the welfare effects of these place-based policies can be quite large in the presence of labor market frictions and high levels of unemployment (Kline and Moretti, 2013; Austin, Glaeser, and Summers, 2018). Bearing these two points in mind, the question is whether CasMez was targeted at fast-growing areas which were initially deprived or those with higher growth potential from the very outset. To test this, I split municipalities into different subsamples according to the median of three pre-CasMez measures of *under-development*. In particular, I use illiterate rate, share of population active in agriculture and share of households with no access to water.

Table 2.5 compares the baseline results shown in table 2.3 with the results obtained by estimating 2.3 on different subsamples of municipalities according to median splits mentioned above. The results show that the coefficient is positive and large only in municipalities with below median values of underdevelopment regardless of the meausure used to proxy for it. Most importantly, standard errors also do not increase a lot indicating that the statistical significance behind the baseline results is also driven by precisely these areas. CasMez funds were thus increasingly targeted not only at areas which were growing faster, but particularly at those with lower initial levels of underdevelopment. These results are partially confirmed by splitting municipalities according to the 1951 value of JPC and plants per capita (PPC).

Table 2.6 shows that the IV coefficients are positive in each group of municipalities, but only significant for those with initial higher values of JPC or with initial lower values of PPC. While the former result is easily reconcilable with the findings presented in table 2.5, the latter is less easy to interpret. In general, however, PPC is a problematic measure of development as it also a measures spatial dispersion of enterprises.

To further understand how the effect varies along the distribution of initial levels of under-development of the municipalities, I also split the sample according

Table 2.5: Heterogeneity I: pre-CasMez underdevelopment
Outcome variable: growth rate of CasMez funds

	All	Illiterate rate		%of pop active in agriculture		% households w/o water	
above/below median		below	above	below	above	below	above
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δjpc	0.950***	1.478***	-0.033	1.066***	0.036	1.055***	-0.507
	(0.292)	(0.394)	(0.480)	(0.329)	(0.492)	(0.326)	(0.739)
Time FE	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES
Time FE*Controls	YES	YES	YES	YES	YES	YES	YES
Population weights	YES	YES	YES	YES	YES	YES	YES
Observations	11240	5620	5620	5620	5620	5620	5620
R-squared	0.520	0.570	0.459	0.566	0.363	0.552	0.339
Clusters	2810	1405	1405	1405	1405	1405	1405

Notes: The table reports coefficients from IV regressions. The first column reports the baseline results as in table 2.3. Columns 2-7 report the coefficient from the same regressions on subsamples split by the median of three measures of underdevelopment: share of illiterates, share of population active in agriculture and share of households with no access to water. The instrument is the weighted average of growth of jobs per capita in 3-digits sectors in the whole South. The controls included are measured at the beginning of the sample period and are: share of resident population that is illiterate, share of households with no access to water, log of resident population, log of municipality area in squared km, population density, share of resident population active in agriculture, average altitude in metres, a dummy for coastal areas. The units of observation are municipalities in 5 points in time. Standard errors clustered at municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.6: Heterogeneity II: pre-CasMez industrial conditions
Outcome variable: growth rate of CasMez funds

	All	JPC 1951		PPC 1951	
above/below median		below	above	below	above
	(1)	(2)	(3)	(4)	(5)
Δjpc	0.950***	0.410	1.216***	0.799***	1.017
	(0.292)	(0.451)	(0.358)	(0.290)	(0.696)
Time FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Time FE*Controls	YES	YES	YES	YES	YES
Population weights	YES	YES	YES	YES	YES
Observations	11240	5620	5620	5620	5620
R-squared	0.520	0.440	0.562	0.588	0.453
Clusters	2810	1405	1405	1405	1405

Notes: The table reports coefficients from IV regressions. The first column reports the baseline results as in table 2.3. Columns 2-5 report the coefficient from the same regressions on subsamples split by the median of jobs per capita (JPC) and plants per capita (PPC). All other notes of table 2.5 apply. Standard errors clustered at municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

to quintiles of JPC in 1951. Strikingly, table 2.10 in appendix 2.B shows that the significant and strong correlation between growth rates of CasMez funds and JPC exists only in the top quintile of initial local conditions. This suggest that the baseline results are entirely driven by places which were already much better off when CasMez started its funding activity.

The final dimension of heterogeneity I investigate is the way of measuring CasMez intervention. The baseline results show that the flow of money grows more in areas that are growing faster but are not informative about the way this flow is generated. As the allocation of CasMez funds was plausibly happening at a higher frequency than decades, the growth in the flow of money could derive from two scenarios: i) CasMez was funding *larger* investments in better performing municipalities or ii) CasMez was funding *more* investments in better performing municipalities. To disentangle between these two scenarios, I replicate the analysis presented in section 2.5.1 with different outcome variables. One is the inverse hyperbolic sine transformation of the total amount of projects per capita (*count*) and the other one is the ratio between the flow of money and the count (*size*)[17] .

Table 2.7 compares the baseline results as in table 2.3 to the results obtained by using the growth rate of *count* and *size* instead of *funds*. In line with scenario i), JPC growth seems to be uncorrelated with the changes in the number of projects but significantly and positively correlated with the growth in the average size of projects. Table 2.8 reproduces the same analysis, but with the outcomes measured as levels and not as growth rates. Again, while higher JPC growth rate is associated with higher number and larger average size of the projects, the number of projects is unaffected. Overall, these results suggest that CasMez targeted municipalities which were expanding more, by providing more *funds per project* rather than more projects.

[17]Results are robust to logarithmic transformations.

Table 2.7: Heterogeneity III: growth of amount, count and average size of projects

	Funds growth		Count growth		Size growth	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Δjpc	0.400***	0.950***	0.186***	0.004	0.132***	0.302*
	(0.046)	(0.292)	(0.023)	(0.219)	(0.029)	(0.167)
Time FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Time FE*Controls	YES	YES	YES	YES	YES	YES
Population weights	YES	YES	YES	YES	YES	YES
Observations	11240	11240	11240	11240	11240	11240
R-squared	0.541	0.520	0.692	0.689	0.240	0.229
Municipalities	2810	2810	2810	2810	2810	2810
IV F-stat		39.49		39.49		39.49

Notes: The table reports coefficients from OLS and IV regressions. Odd columns show OLS estimates while even columns show IV ones. The first two columns reports the baseline results as in table 2.3. Columns 3-6 reports the coefficient from the same regressions with the outcomes being the growth rate in the number of projects in columns 3 and 4, and the growth rate in the average project size in column 5 and 6 . The instrument is the weighted average of growth of jobs per capita in 3-digits sectors in the whole South. The controls included are measured at the beginning of the sample period and are: share of resident population that is illiterate, share of households with no access to water, log of resident population, log of municipality area in squared km, population density, share of resident population active in agriculture, average altitude in metres, a dummy for coastal areas. The units of observation are municipalities in 5 points in time. Standard errors clustered at municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.8: Heterogeneous results IV: levels of amount, count and average size of projects

	Funds		Count		Size	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Δjpc	0.384***	1.816***	0.242***	0.397	0.126***	0.602***
	(0.052)	(0.581)	(0.031)	(0.464)	(0.022)	(0.213)
Time FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Time FE*Controls	YES	YES	YES	YES	YES	YES
Population weights	YES	YES	YES	YES	YES	YES
Observations	11240	11240	11240	11240	11240	11240
R-squared	0.648	0.576	0.608	0.607	0.309	0.195
Municipalities	2810	2810	2810	2810	2810	2810
IV F-stat		39.49		39.49		39.49

Notes: The table reports coefficients from OLS and IV regressions. Odd columns show OLS estimates while even columns show IV ones. The first two columns reports the baseline results as in table 2.3, but with the outcome measured as level. Columns 3-6 reports the coefficient from the same regressions with the outcomes being the number of projects in columns 3 and 4, and the average project size in column 5 and 6. All other notes of table 2.7 apply. Standard errors clustered at municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

2.6 Conclusion

Place-based policies are often used by governments as interventions to foster economic growth in lagging regions. A growing literature has studied the consequences of these policies theoretically and empirically (see for instance Ham et al., 2011; Busso, Gregory, and Kline, 2013; Kline and Moretti, 2014a; Duranton and Venables, 2018). The evidence suggests that place-based policies can generate non-negligible distortions in economic behaviour so that in aggregate terms the welfare effect are ambiguous. Especially when designed on the basis of equity arguments, it is unclear why this type of policies should be preferred to person-based interventions (Neumark and Simpson, 2015).

I propose shifting the attention to the actual implementation of these policies and identifying whether there is evidence for any economic rationale at all. In this paper, I study if and how local economic growth influenced the allocation of CasMez, a very large place-based policy which took place in Southern Italy for more than 40 years after WWII. I build a novel municipality-level dataset covering the whole period of CasMez activity and combine it with relevant socio-economic variables from the industry and population censuses. Since local economic growth is endogenous to the policy, I instrument local labor demand shocks with the aggregate growth at the industry level weighted by local industry structure in the baseline period, following Bartik (1991). I find that areas with better growth prospects and with higher growth rates received more funds during the entire period. Moreover, I demonstrate that the spatial distribution of projects is independent of local growth rates and that the effect is coming from bigger investments being allocated to better performing municipalities.

This paper constitutes the very first attempt to systematically investigate the allocation of the universe of CasMez projects from a micro perspective. I contribute to the long-standing debate on CasMez and provide robust evidence that the targeting mechanism differed from the original intentions and often did respond to local economic shocks albeit in the favour of prospering areas. There are several ways of interpreting my findings. First, policy-makers might have intentionally targeted better performing municipalities in the hope of getting higher returns per unit of public money invested. Secondly, the common narrative regards CasMez as an *extra-ordinary* intervention in the original plans but as an *ordinary* one in practice, meaning that it had to substitute standard national policies (Svimez, 2015). Finally, these findings might be consistent with predictions from political economy models that good economic times in developing countries encourage fiscal profligacy and/or rent-seeking activities(Ilzetzki, 2011; Alesina, Campante, and Tabellini, 2008). Even though the data does not allow disentangling these different channels, my results suggest that the allocation mechanism followed by CasMez might have contributed to its partial failure. This is motivated by evidence that marginal increases in employment induced by shocks to labor demand can be very high in areas were unemployment has historically been low (Bartik, 2014; Austin, Glaeser, and Summers, 2018). There is a possibility then that CasMez might have proven more effective had it targeted deprived areas rather than those which were already prospering.

2.A Data

2.A.1 CasMez and municipality borders

In this section I briefly describe how I have constructed municipality-level data from the project-level data on CasMez. The data covers the whole period of CasMez activity and is the basis for other research projects (see chapter 1 of this thesis). The raw dataset consist of four datasets, one for each type of CasMez' project: industrial grants, industrial loans, public works and special projects. For the the first two datasets each project entry is associated to a firm and its location. Matching the project to municipality is then straight-forward. However, for public works and special projects not all entries are matched to a single municipalities. Some entries are in fact recorded as *pluricomunali*, i.e. multi-municipalities. The next section explains how I deal with them.

For determining the time dimension of the projects I use a combination of the date-related fields available in the datasets. In fact, for each projects there is a starting date, an end date and sometimes an acceptance date. As often there are missing values I use the earliest date between starting and acceptance date to determine the year in which the project is allocated to the municipality. Projects for which no date is available are excluded from the sample.

A critical part of the construction of the dataset is that projects are recorded for municipality borders as of 2011. However, there have been many changes to these borders over 40 years of history and it is important to correctly track them, also to then link CasMez' dataset to the censuses. For consistently defining a municipality identifier, I keep the largest units over time and, if there are aren't, I keep the oldest unit. In particular, I follow these criteria:

- If a municipality is created after 1951 out of a pre-existing municipality I substitute the current identifier with the pre-existing identifier;
- If a municipality is created after 1951 out of multiple pre-existing municipalities which do not exist anymore then I attribute the new identifier to pre-existing municipalities in the census data,

- If a municipality is created after 1951 out of multiple pre-existing municipalities which still exist then I cannot track them. This is a very rare case anyways (15 municipalities in my sample).

2.A.2 Allocation of *pluricomunali* projects to municipalities

General criteria Even if *multi-municipality* projects consist of only 10% of the overall number of projects, they make up 48% of the money invested in public works and 37% of overall money during the 40 years of activity of CasMez. I allocate more than half of these projects, ending up covering 85% of public works money and almost 90 % of overall money. To do so I employ first a string routine in the main dataset and then manual allocation. Once I have identified the target area, I use both arithmetic mean and weighted-by population mean to distribute the projects funds across municipalities. Results provided do not change when using either measures.

The routine is based on extraction of municipalities' names from string-variables describing the projects. However, this is possible only for 12% of the overall money, as many projects have an incomplete list or refer to a geographic areas only. I then manually identify the involved municipalities either by manual search of the area of interest or, when this is not possible, through GIS[18]. To make the manual allocation more efficient, I start from largest projects. For both routine-based and manual allocations, I adopt the following general criteria:

- Include incomplete works
- Stick to the province mentioned in the dataset if:
  - work seems to affect other provinces;
  - need to disentangle between subsets of municipalities;
  - the amount of municipalities involved is not clear but a main one is specified in the project name;
- For aqueducts:

[18] I thank Rory Nealon for his extremely precious help on this.

  - if the project does not specify sections of the aqueduct scheme, I obtain list of irrigated municipalities from official documents of ISPRA (if available) or from web search (often it is the website of company dealing with the aqueduct at the moment);
  - if the project does not specify sections of the aqueduct scheme but specifies an area (not in administrative terms) of intervention then I do a web search of municipalities being part of the area;

- If projects seems large enough and the province involved is specified, I give to all municipalities in that province.

GIS routine I extracted geographical features from OpenStreetMap (OpenStreetMap contributors, 2017) through overpass-turbo and analysed them with QGIS, via the following routine:

- Identify the main geographic keywords for several types of geographic references:
  - River: one keyword only which is the river name
  - Aqueduct: at least two keywords which identify the aqueduct's path
  - Dam: one keyword that is often the name of the artificial lake.
  - Road: two keywords that represent the municipalities at the extremes of the road.
- Locate the geographic features of the keywords in OpenStreetMap
- Query the feature(s) in Overpass turbo and export the data as a geojson and clean in QGIS
- For features that are lines calculate the share of length for each municipality and allocate the money with respect to this share
- For polygon features calculate the area of the different clipped polygons and then calculated the share for each municipality and allocate the money with respect to this share

2.B Additional results

Table 2.9: IV results with spatial trends
Outcome variable: growth rate of CasMez funds

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δjpc	0.950***	1.179*	0.865**	0.916***	0.915***	0 0.320	0.895**	0.959***
	(0.292)	(0.629)	(0.374)	(0.353)	(0.303)	(0.341)	(0.356)	(0.295)
Municipality trends	NO	YES	NO	NO	NO	NO	NO	NO
CZ trends	NO	NO	YES	NO	NO	NO	NO	NO
Province trends	NO	NO	NO	YES	NO	NO	NO	NO
Region trends	NO	NO	NO	NO	YES	NO	NO	NO
CZ X time trends	NO	NO	NO	NO	NO	YES	NO	NO
Province x time trends	NO	NO	NO	NO	NO	NO	YES	NO
Region X time trends	NO	NO	NO	NO	NO	NO	NO	YES
Observations	11240	11240	11240	11240	11240	11240	11240	11240
R-squared	0.520	0.497	0.547	0.532	0.526	0.749	0.605	0.542
Clusters	2810	2810	2810	2810	2810	2810	2810	2810
IV F- stat	39.49	21.41	30.90	31.77	35.85	25.68	34.83	40.84

Notes: The table reports coefficients from IV regressions. The first column reproduces the baseline results as in table 2.3. Columns 2-8 report the coefficients obtained by adding different fixed effect to control for non-linear trends. The instrument is the weighted average of growth of plants per capita in 3-digits sectors in the whole South. All columns include time fixed effect and controls interacted with time fixed effects. Observations are weighted by population in 1951.The controls included are measured at the beginning of the sample period and are: share of resident population that is illiterate, share of households with no access to water, log of resident population, log of municipality area in squared km, population density, share of resident population active in agriculture, average altitude in metres, a dummy for coastal areas. The F-stat from the first stage is reported. The units of observation are municipalities in 5 points in time. Standard errors clustered at municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.10: Heterogeneous results: initial JPC-quintiles

Outcome variable: growth rate of CasMez funds

	(1) OLS	(2) IV
Δjpc	0.134***	0.241*
	(0.027)	(0.124)
Δjpc * q2	0.139***	0.116
	(0.053)	(0.083)
Δjpc * q3	0.156***	0.173
	(0.050)	(0.107)
Δjpc * q4	0.221***	0.249*
	(0.050)	(0.145)
Δjpc * q5	0.264***	0.911***
	(0.076)	(0.255)
Time FE*Controls	YES	YES
Population weights	YES	YES
Observations	11240	11240
R-squared	0.405	0.388
Municipalities	2810	2810

Notes: The table reports coefficients from OLS and IV regressions. The independent variable is interacted with dummies for each quintile of the distribution JPC in 1951.The instrument is the weighted average of growth of plants per capita in 3-digits sectors in the whole South. The controls included are measured at the beginning of the sample period and are: share of resident population that is illiterate, share of households with no access to water, log of resident population, log of municipality area in squared km, population density, share of resident population active in agriculture, average altitude in metres, a dummy for coastal areas. The units of observation are municipalities in 5 points in time. Standard errors clustered at municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.11: JPC and PPC by sector: north vs south-every 1000 inhabitants

Code	Description	JPC North	JPC South	PPC North	PPC South
2010	MET MINERALS	0.496	0.803	5.284	6.668
2020	NON MET MIN.	2.186	2.686	198.2	217.6
3010	FOOD & BEV	8.285	9.906	1586	2699
3020	TOBACCO	0.946	1.634	11.28	36.85
3030	LEATHER	1.193	0.356	182.3	141.7
3040	TEXTILE	23.06	1.813	1338	340.3
3050	CLOTHES & SHOES	10.74	8.161	5431	5505
3060	WOOD & FURN.	8.387	5.613	2849	2970
3070	PAPER GOODS	2.214	0.333	73.26	19.29
3080	EDITING AND PRESS	2.636	0.546	233.6	106.6
3090	PHOTO & VIDEOS	0.343	0.170	156.3	123.0
3100	METALLURG	5.232	0.724	37.58	3.728
3110	MECHANICS	30.87	6.067	3649	2423
3120	NON MET. MIN. PROD	6.418	2.610	459.3	456.8
3130	PETROCHEM	6.845	1.211	212.4	104.0
3140	RUBBERS	1.554	0.0516	57.69	20.51
3150	PLASTICS	2.027	0.154	140.5	29.54
4010	CONSTR.	16.13	7.995	1298	650.4
5010	ELECT. & GAS	2.365	1.144	154.0	108.1
5020	WATER	0.281	0.334	68.18	50.98
6010	WHOLESALE TRADE	7.602	3.272	2373	1131
6020	RETAIL TRADE	26.59	20.17	13152	12282
6030	HOSPITALITY	9.326	4.521	4149	1988
7010	TRANSPORTS	13.10	8.701	1838	1533
7020	COMMS	3.473	2.206	225.0	207.7
8010	BANKING	2.610	1.459	265.7	131.4
8020	INSURANCE	0.792	0.127	111.0	44.17
9010	SERVICES TO FIRMS	2.801	1.969	1432	899.8
9020	ENTERTAINMENT	1.336	0.796	290.6	182.4
9030	HYG & CLEANING	3.279	3.662	1759	1709

Chapter 3

Do R&D subsidies to start-ups incentivise innovation?

3.1 Introduction

Public support aimed at fostering private innovation activities is a common practice in almost every OECD country. In 2015, governments across the OECD invested on average the equivalent of nearly 0.17% of GDP in direct funding for R&D (OECD, 2017)[1]. The instruments typically used to promote R&D include direct grants or loans, governmental labs, R&D public contracts with private firms and tax incentives. In particular, governments devote large resources to support smaller firms or start-ups. The economic rationale behind this is that start-ups face higher marginal cost of innovation and stronger credit constraints (Hall, 1996; Hall and Lerner, 2010). This is particularly true for firms operating in the high-tech sectors (Guiso, 1998).

The empirical literature on the effects of public funding for R&D activities is in general very vast and results differ significantly across types of programmes or countries (for instance see Lerner, 2000; Almus and Czarnitzki, 2003; Einiö, 2014). High-tech start-ups have not received a lot of attention so far[2], even though they can be of particular interest. First, start-ups have higher application costs and fewer application opportunities than regular recipients of public grants. Established companies often regard subsidies as a regular source of external cash flow rather than as an incentive, whereas for start-ups they can be a one-shot, potentially transformative opportunity. Young firms are then less likely to fund inframarginal projects. Secondly, since both marginal costs of innovation and credit constraints are higher for smaller firms, their response to receiving a subsidy can be interpreted as an upper bound of the response of larger or more mature firms.

This paper provides insights into the causal effect of public R&D support to start-ups. For this end, I exploit a unique quasi-experimental setting in the allocation of a direct subsidy introduced by the Italian government in 2009 which was targeted at high-tech start-ups. The scheme offered refund of up to 50% of the total costs of highly technological projects and comprised average transfers of almost 1 million euros per firm. I estimate the impact of this programme on innovation

[1] The average among countries in the top quintile of the funding distribution goes up to 0.36%.
[2] See, for a notable exception, Howell (2017).

activities of the subsidised firms using a Regression Discontinuity Design (RDD)[3]. In particular, I exploit the score that each applicant firm received after applying for the grant to compare the firms that just received the grant to the ones that did not.

One of the main challenges of using RDD is to establish that the assignment to the treatment around the cut-off is as good as random. In the context of score-based rankings of applications, this assumes that marginal applicants cannot signal or manipulate their position around the threshold. Virtually all related papers using score-based rankings in an RDD setting have assessed the validity of this crucial assumption by investigating discontinuities in the number or characteristics of the applicants on either side of the cut-off (see for instance, Bronzini and Iachini, 2014). However, even if scoring procedures are often ruled by precise guidelines, it is in practice exactly around the threshold that scoring committees can exert their discretionary power. In this case, the observations may not be randomly allocated around the cut-off and instead may be determined by unobservable factors that could also affect the outcome variable. In other words, the presence of a cut-off score that is exogenous to the applicants does not necessarily rule out non-random sorting as the cut-off may still be endogenous to the policy-maker. Such type of sorting may go undetected by standard tests of balance across treatment and control groups.

In this paper, I address this issue by exploiting the interaction between the specific scoring procedure of the programme and the *exogenous* supply of funds. The score determining the treatment was assigned to each application by a committee made up of experts of technological innovation. The crucial feature of this setting is that the cut-off score was not determined *ex-ante*. Rather, the score above which firms received subsidies was determined by the supply of funding allocated to this programme by the central government. Consequently, the committee was arguably blind to the cut-off and unlikely to determine *ex-ante* which firms would receive the grant. In fact, some firms did not receive the subsidy because of the funding constraint although they obtained exactly the same score as other treated firms.

I quantify the impact of the subsidy in this setup by using balance sheet

[3]This is not the first paper using RD for evaluating the impact of public funds on private firms. See, for instance Cerqua and Pellegrini (2014) or Bronzini and Iachini (2014).

and patenting data and comparing R&D inputs and outputs for firms on both sides of the threshold. In particular, I focus on the share of intangibles out of total assets (*intangibles' intensity*) as a proxy for R&D expenses and on applications for patents as a proxy for R&D outcome. I measure the causal effect of the grant using both parametric and non-parametric estimations. My results show that the average intensity of the intangibles after the treatment is not different between treated and untreated firms. However, there is a positive and significant effect in the first two years after the publication of the ranking. In these years, firms that received the grant devoted a 20% higher share of total assets to intangibles. I do not find any significant effect on patenting activity, even if there is partial evidence for a *decrease* in patent applications after the treatment in treated firms. The results are exposed to several limitations deriving from the sample size and measurement issues but suggest that the subsidy might have increased investments in R&D at least in the short-run.

The remainder of the paper is organised as follows. Section 3.2 describes the predictions of the theory and relevant empirical findings to outline the contribution of this study. Section 3.3 provides some information on the grant programme and section 3.4 describes the datasets used in the analysis. Section 3.5 explains the identification strategy and section 3.6 presents the main results. Section 3.7 interprets the results and provides the conclusions.

3.2 Literature Review

The economic rationale for public support of innovation lies in two main potential market failures, namely underprovision of investments in innovation and credit constraints (Arrow, 1962; Nelson, 1959). The first market failure arises because knowledge is a public good for which the social marginal return is higher than the private one (Griliches, 1986; Hall, 1996). This can be combined with the stylised fact (Hall and Lerner, 2010) that R&D spending at firm level usually behaves as it had high adjustment costs, especially because at least half of this spending translates into higher remunerations of human capital[4] so the private marginal return required in equilibrium might be too high. The government can then decrease the marginal cost of innovation projects through direct subsidies or tax credit. When it comes to small and medium enterprises (SMEs) or start-ups, this argument is strictly connected to market power: the present value of an R&D investment is potentially a negative function of firms' size or market position because it is harder for the firms to prevent other competitors from having access to the innovative results[5].

However, even if innovations could be fully appropriated, information asymmetries between entrepreneurs and external investors are particularly high when it comes to intangible capital for three main reasons. First, research investments can be extremely risky and the cost of external capital can be too high for the firms. This is again especially true for small firms that can easily be deterred from undertaking projects with a positive net present value (Hall, 2002). Second, the strategic nature of this kind of investments can increase the degree of asymmetric information when the firm is keen to conceal the innovative characteristics of its project. Finally, intangible capital cannot be used as collateral. The government can intervene by partly substituting the private capital market and providing funding for the firms.

Government intervention can also generate several positive externalities. If the innovation process involves upgrading general research facilities, a government subsidy would decrease the fixed cost of the firms to undertake other private R&D projects. Also, spillover effects can arise both within and across firms[6] and the ability of the firm to obtain public funding may have positive signalling effect on the private capital market[7].

The effectiveness of subsidies, however, strongly depends upon policy makers' ability to target marginal projects which is to say those projects which would

[4]Firms try to avoid having to lay off knowledge workers. This implies a tendency to smooth R&D spending over time.

[5]The innovation literature looks at the effect of patenting normative on innovation activities. See, among others, Moser (2005).

[6]The predictions in this case are actually quite ambiguous as I discuss when reviewing the empirical literature.

[7]The validity of this prediction is of course substantially dependent on the type of funding and on public administration's reputation and credibility.

have not been undertaken in absence of the intervention. Noticeably, the government faces the same asymmetries of the private capital market. Every firm may de facto have an incentive to apply for public grants in spite of its capacities in terms of internal funding or appropriability. If the government reaches infra-marginal projects, then public investments crowd out private ones and the equilibrium investment level does not change in comparison to a scenario without government intervention. This is very likely to be the case if policies are implemented according to a picking-the-winner criterion, especially given the reputation benefit that policy makers might gain from a successful subsidy programme. Similarly, if the incentive is based on a helping-the-losers strategy (for instance, in the attempt of encouraging R&D investments in small or more financially constrained firms), inefficient firms might be supported. Moreover, Goolsbee (1998) finds that the crowding-out effect can also involve private inventive activity even in firms that do not directly receive support. As the supply of scientific and engineering skills is inelastic, a large public subsidy can have a general equilibrium effect on the prices of research inputs and the funding is more likely to be routed into rewarding human capital than increasing innovation.

According to economic theory then, the effect of public R&D support to private firms is ambiguous and that is why the literature developed around this issue on a mainly empirical ground. However, the question is as difficult to address empirically as it is theoretically. Several methodologies, datasets and contexts have been employed in the literature especially in the last two decades. The main obstacle to a credible identification strategy is that subsidised firms are not randomly chosen[8]. Government agencies often base funding allocation on factors that the researcher does not observe and that can be correlated with the future performance of the firms. Moreover, R&D activities are intangible and identifying them within a firm can be challenging when working with firm level data or misleading when working with survey data. Measuring innovation with patenting data only is also believed

[8] As mentioned above, I am focusing here on direct funding. Tax credit or other indirect fiscal tools meant to foster R&D investments are commonly based on more "automatic" selection criteria that still create similar endogeneity issues.

to be an incomplete approach (Nagaoka, Motohashi, and Goto, 2010; Moser, 2012, 2013). Many studies focus on innovation outputs (such as the general performance of the firm or productivity) rather than inputs to overcome the aforementioned measurement issues. Finally, an important threat to the estimation of the effect of the subsidies is the presence of spatial and industry spillovers. In particular, this can be a potential problem in many matching-based methodologies; by using not subsidised firms to create a counterfactual for subsidised ones might lead to neglect that a negative(positive) estimated effect could actually prove the success (failure) of the subsidy because of the presence of positive (negative) spillovers.

There is a very vast empirical literature to which the current study makes a contribution. David, Hall, and Toole (2000) and Zúñiga-Vicente et al. (2014) amongst others have brought together and analysed all the empirical studies of the last 30 years in order to design a more precise path for future research. Here, I focus on the works that are more closely related to mine, firstly in terms of identification strategy and secondly in terms of institutional context. Lach (2002), using survey and performance data on Israeli manufacturing firms and a Difference-in-Difference, finds that R&D grants increase innovative activities in small firms but not in large ones. At the time, the approach was novel and the dataset broad; however, the counterfactual for the subsidised firms is made up of both firms that applied for but did not get the grant and firms that did not apply for the grant. Noticeably, firms that apply for R&D subsidy can systematically differ from firms that do not apply. Busom (2000) finds evidence for significant crowding-out effects when estimating a structural model on a sample of Spanish firms. Her contribution lies in the estimation of a participation equation that formalises both public agencies' goal and applicants' characteristics. Einiö (2014) employs a well-conceived identification based on the variation in government funding arising from the allocation of EU Regional Development Funds in Finland. He then identifies the causal effect of the programme, exploiting the variation in the probability of R&D support programme participation. He finds positive effects on both innovation and productivity. The results from Finland might not be, however, very representative. Finland, Sweden and

Denmark are the only EU Members with an R&D expenditures-GDP ratio larger than 3%[9].

Few papers have looked at the effect of public funding for R&D on firms in Italy. Using a matching model, Merito, Giannangeli, and Bonaccorsi (2010) analyse a Special Grant awarded in 2000 to some innovative firms and found no effect on employment, sales and productivity. Employing non-parametric matching, Carboni (2011) finds that recipient firms achieve more private R&D than the counterfactual. The closest papers to this work are Bronzini and Iachini (2014) and Bronzini and Piselli (2016). They apply RDD to a subsidy with an allocation procedure similar to mine and find a positive effect on small firms only, also in terms of patenting. Two factors distinguish my work from theirs. First, I look at a national programme and not a regional one. Second, and most importantly, their cut-off score is predetermined. The authors show the inability of the firms to manipulate the score but cannot rule out the discretionary power of the committee.

3.3 Context

The Innovative Start-ups Industrial Research Program[10] was launched by the Italian Ministry for the Economic Development on July 2009 with the intention to sustain experimental development and industrial research projects of start-ups in high and medium-high technological sectors. The Ministry allocated 55 million Euros, 20 millions of which originated from the European Structural Funds and were earmarked to firms active in the four Convergence Regions[11].

Figure 3.1 shows the timeline of events from the call for applications to the publication of the final ranking. Firms with less than 5 years of activity could apply, also jointly with research institutes, for the grant by presenting projects with specific budget plan and technical details not sooner than two and no later than five months after the call for applications was announced. The eligibility of the projects

[9]That is even above the target set by Lisbon Strategy.

[10]Decree 7 July 2009 - FIT, alta e media tecnologia. Published on 25 July 2009.

[11]These are the Regions whose per capita GDP is less than 75% of the EU and they are Calabria, Campania, Puglia and Sicilia.

was related to three main criteria. First, the projects had to be strictly linked to development of product and/or process innovations in the following technological sectors: biotechnologies, ICT, innovative materials, robotics and energy innovation. Secondly, the request for the grant had to be related to the coverage of specific R&D costs: long or short term contracts for hiring researchers, purchase of tools and machinery, use of consulting services, fees for filing of patents, raw materials and general expenditures up to 30% of the personnel cost. Finally, the projects had to start *after* the submission of the application. This is a crucial feature of the programme, that rules out crowding-out effects *within* a firm.

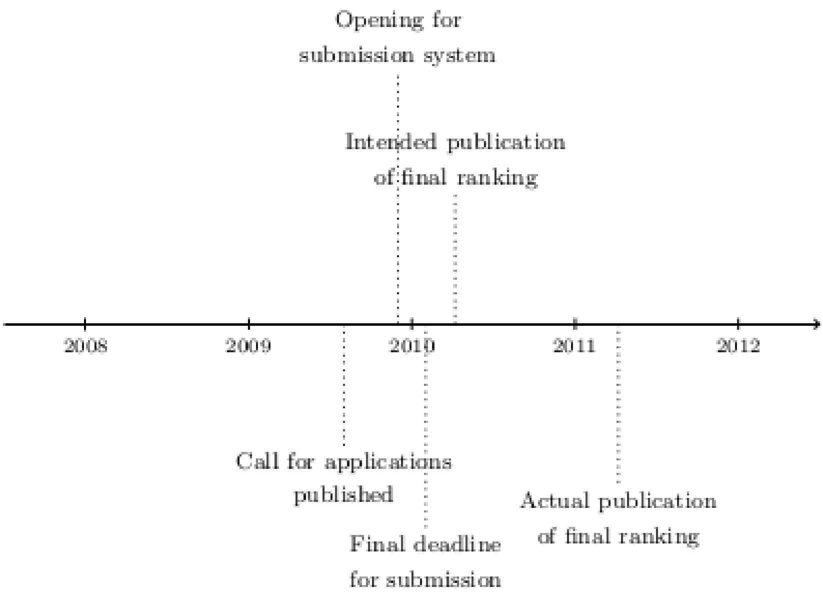


Figure 3.1: **Timeline of application process**

Once the projects were submitted, the Ministry selected a committee of 5 people listed in the official register of experts in technological innovation[12]. The register was formed in 2006 through a public competition targeted at academics, members of public research institutes and high-qualified individuals with experience of at least 10 years in research evaluation and management in different technological

[12]See the Ministry's decree 7/4/2006.

areas. The committee compiled a ranking of projects according to the following scoring criteria:

- maximum of 15 points for the degree of innovation of the project;
- maximum 5 points for the past R&D activity[13];
- 5% bonus increase for product innovation or partnerships with research institutes or female ownership of the firm[14].

Noticeably, the score was mainly project-specific rather than firm-specific, indicating again that the committee did not have a priori preferences over which firms to subsidise. Up to approximately the 150th position of the ranking, all firms were given the maximum score for the relevance of R&D expenditures in past financial years, thereby suggesting that this criterion was almost negligible in the scoring procedure. Between projects receiving equal scores, the ranking was determined on the basis of economic efficiency; thus the cheaper projects received a higher ranking. The grant was finally assigned to the highest ranked projects up till the point wherein available national funds were used up. The most relevant feature of this procedure is that the committee could not know, ex-ante, how the score would have affected the application outcome of the projects.

The final ranking was announced on April 2011, with one year delay with respect to the intended date: out of 411 presented projects, 65 were selected to receive public support. The grant was substantial. In fact, it could cover costs up to 2 million Euros per firm. Awardee firms had to start the project within 6 months from ranking publication if they wanted to prevent revocation of the grant. The support was granted either through direct funding or through a loan with favourable credit terms[15]. The direct subsidy could cover up to 20% of eligible costs for large firms, 30% for medium-sized firms and 40% for small firms[16]. The loan

[13]Measured as R&D expenditures reaching 15% of operative expenditures in one of the 3 years preceding the application.

[14]In particular firms needed to have contracts with research institutes for at least 20% of the eligible costs or projects needed to be jointly presented with research institutes.

[15]According to a dialogue with Ministry's officers, this was around 1.2% interest rate.

[16]The reference framework to define firms' size is given by the European committee (L 124/36,

could instead cover up to 50% of costs for all firm sizes. Overall, the total cost of admitted projects was of 130 million of Euros, 55 of which was fully covered by the public intervention.

Other features of the programme are relevant to the identification strategy presented in this work. First, this subsidy was operating at a national level. This is a rare characteristic as most of the interventions to innovation activities have been in the last decade decentralised to local or federal governments. From a methodological point of view, the most crucial benefits of a national programme over a regional one are that: i) national programmes generate less spillovers of the treatment within the sample of *applicants*[17]; ii) national programmes are more centralised and less likely to create room for informal relationships between candidates and committee[18]. Finally, firms could not apply to other funding (neither national nor regional ones) or receive any other kind of public aid. This allows me to isolate the impact of this intervention.

3.4 Data

This work is based on a novel panel dataset of the firms that applied for the grant. To construct this dataset I have used three data sources. First, I have obtained the administrative dataset of the Ministry of Economic Development that contains information about firms, presented projects, detailed score, cost of the project, grant amount and main applicant in case of joint applications. Secondly, I have collected firm-level data for 9 financial years between 2008 and 2016 from the AIDA database offered by Bureau Van Dijk. AIDA contains comprehensive balance sheet information and has a wide coverage of companies in Italy. Finally, I have constructed a dataset with the universe of patent applications and publication dates of the firms

2003): small firms have less than 50 employees and either turnover or Balance Sheet total less than 10 million Euros. Medium firms have less than 250 employees and either turnover less than 50 million or Balance Sheet total of less than 43 million.

[17]On the other hand, as for regional programmes, the behaviour of the not-funded firms is unknown.

[18]A national intervention can, however, be exposed to political pressures to make geographically diverse awards.

in the sample between 2007 and 2017. In order to do so, I have used web-scraped techniques to systematically search for the firms' names as assignee or inventors of the patents.

The final sample obtained by merging these three sources together excludes partnerships and research institutes that are not covered by AIDA and consists of 361 firms, that correspond to 88% of the applicants. For joint applications, I have considered the data related to the main applicant as recorded in the administrative dataset. The coverage of successful applicants is slightly lower but large enough as there is data for 55 out of 65 subsidised firms. The final panel is not balanced as 44% of the firms were estabilished between 2008 and 2009 and 12.5% were already inactive in 2013. Moreover, even if the firms survived, not all entries are always reported in AIDA; thus I can consistently measure the main outcome in every year for only 70% of the surviving firms. Overall, when I consider the balanced panel that includes only firms that exist and have balance sheet data of good quality in the 2009-2016 period, the sample drops to 183 firms. Figure 3.2 shows how the number of firms with good data quality drastically varies over years. For statistical power but also as the survival of firms and quality of data can be an outcome, I implement the main analysis on the unbalanced panel, but I carry out some robustness checks on the balanced sample.

To measure innovation, I look at both R&D inputs and outputs. AIDA does not provide the notes for the accounts in which firms are obliged to describe the R&D activities. I then proxy R&D expenditures, or inputs, with the investments in intangibles fixed assets. According to the law[19], this balance sheet entry can include: startup expenses, R&D and advertising costs, cost of patents, alternative intellectual property rights and other intangible assets. I construct the main outcome variable, namely intangibles' intensity, by scaling the stock of intangible fixed assets in each year by the total amount of fixed assets at the end of the previous accounting year. A concern in using this balance sheet entry is that cost capitalisation, i.e. labelling costs which are not related to capital investment as such, can be a strategic decision

[19]Italian Civil Code, articles 2424-2426.

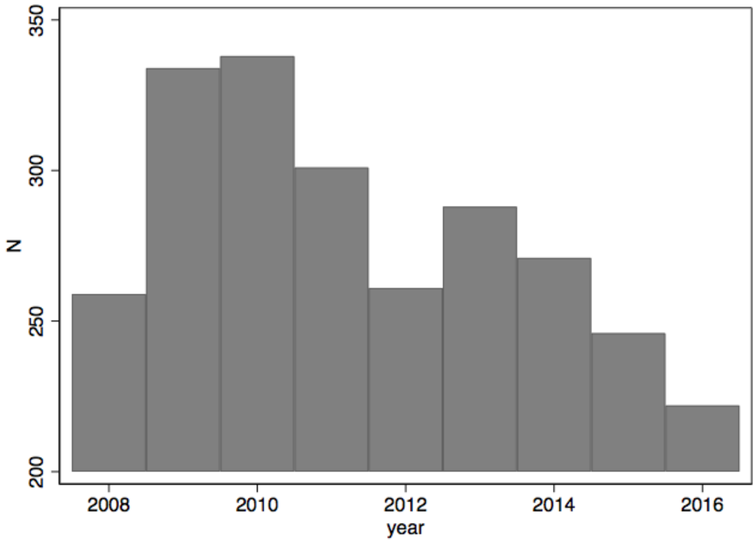


Figure 3.2: **Number of firms with no missing values by reporting year**

for firms and can be influenced by many unobservables.

As the main R&D output, I consider patents published up to July 2018 at the European Patent Office. As patents take on average three years to be published, I consider the date of the application and not of the publication. A limitation of this measure is that I can only observe patent applications that have been successful. As the sample is made of start-ups, only 32% of the firms in my sample have applied for at least one patent. Moreover, this goes down to 23% if I exclude patents filed before 2009. For this reason, I construct a binary variable equal to 1 if the firm applied for at least one patent, to investigate the effect of the subsidy on the *extensive margin* of patenting. Figure 3.8 in the Appendix shows the distribution of patent applications. The largest proportion of firms have filed at most 2 applications, and only very few firms have filed more than 20.

Because of attrition and missing values for balance sheet entries, the balanced panel is very small and equal to 183 observations. Tables 3.1 and 3.2 compare the main variables of interest between the unbalanced and balanced panel. The first table is based on the time period *pre*-publication of the ranking, while the second table is based on the *post*-publication period. These tables provide two main insights. First, there seems to be an *increase* in balance sheet entries after the

Table 3.1: Descriptive statistics by sample: 2009-2010

	Unbalanced			***Balanced***		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
Tangible fixed assets	1323.81	10192.57	684	1836.37	13471.60	366
Intangible fixed assets	614.46	3566.65	685	756.42	4363.51	366
Total assets	5063.25	27671.08	684	7025.83	36050.61	366
Intangibles' Intensity	0.18	0.23	684	0.15	0.20	366
Sales	2745.71	15013.15	682	3419.20	17665.49	366
Value of production	2932.62	15531.28	682	3631.77	18115.62	366
Profit or loss	−100.72	1063.32	684	−153.93	1425.05	366
Time-invariant						
Patents up to 2008	0.74	3.05	722	0.86	3.24	366
Patents, 2009-12	0.95	3.47	722	1.18	3.79	366
Patents, 2013-17	0.21	0.85	722	0.32	1.11	366
Less than 50 empl	0.07	0.26	722	0.10	0.30	366
Less than 250 empl	0.03	0.17	722	0.04	0.19	366
Southern regions	0.25	0.43	722	0.22	0.42	366

Table 3.2: Descriptive statistics by sample: 2011-2016

	Unbalanced			***Balanced***		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
Tangible fixed assets	3661.82	37876.92	1613	5060.45	45801.73	1098
Intangible fixed assets	721.82	3461.39	1613	853.08	4047.36	1098
Total assets	8282.73	56703.00	1613	11051.97	68433.27	1098
Intangibles' Intensity	0.17	0.23	1613	0.15	0.20	1098
Sales	3980.20	22325.36	1613	5385.55	26856.80	1098
Value of production	4380.86	23880.71	1613	5916.58	28725.55	1098
Profit or loss	−267.10	3934.74	1613	−310.67	4312.19	1098
Time-invariant						
Patents up to 2008	0.74	3.05	2166	0.86	3.23	1098
Patents, 2009-12	0.95	3.47	2166	1.18	3.79	1098
Patents, 2013-17	0.21	0.84	2166	0.32	1.11	1098
Less than 50 empl	0.07	0.26	2166	0.10	0.30	1098
Less than 250 empl	0.03	0.17	2166	0.04	0.19	1098
Southern regions	0.25	0.43	2166	0.22	0.42	1098

Notes: These tables show summary statistics for the main balance sheet and patenting data. The balanced sample is restricted to observations for which *Intangibles' intensity* can be observed in each year between 2009 and 2016. The number of observations is equal to the number of firms*number of years.

publication of the ranking in both unbalanced and balanced samples. Secondly, the firms in the balanced sample are noticeably larger and patent more. This is not surprising as larger firms tend to have better reporting practices and, most importantly, have more reporting obligations than smaller firms. The implications of using these two different samples are discussed in more detail in the following sections.

3.5 Identification Strategy

This section describes in detail the identification strategy and its validity in this unique setting. As already mentioned, I use an RDD by exploiting the final score firms received as the forcing variable. As the treatment, which is winning the subsidy, is a deterministic function of the score received, I apply a sharp RDD. Firms that did not get the subsidy but got a score close to cut-off one are a proper counterfactual for the subsidised firms with a score just above the cut-off. If some assumptions are met, then the treatment status around the threshold is randomised as though from randomised experiments (Lee and Lemieux, 2010) and any potential discontinuity gap at the cut-off identifies the causal effect of the treatment. Section 3.5.1 explores the validity assumptions for RDD and tests them in the context of this paper. Section 3.5.2 outlines how the treatment effect is estimated.

3.5.1 Validity of RDD

One of the main challenges in RDD is the potential manipulation of the running variable. However, the setting of the programme studied here allows to rule out any form of non-random sorting around the cut-off. As a matter of fact, the committee was ex-ante unaware of the cut-off score. To be more specific, a precise threshold was actually never determined. More than 10 firms received a score of 18.9 out of 20; however, only 6 of them were assigned the grant because of unavailability of extra funds, subsequent to being ranked from the cheapest to the most expensive project[20]. The cut-off is then likely to be exogenous to firms' characteristics but this can anyway be tested empirically.

First, if the cut-off is not exogenous because individuals are able to determine the value of the score to be on one side of the cut-off, then one might observe a discontinuity in the density distribution of the forcing variable at the threshold. Figure 3.3 plots the density distribution of firms with the score normalised to be centered at 0. The distribution around the cut-off score is smooth. Also, the McCrary test (McCrary, 2008) leads to a t-ratio of 1.1 and thus fails to reject the null hypothesis of continuity of the density.

To check whether firms below the cut-off can be a proper counterfactual for the ones above, I produce RD plots for a bunch of relevant covariates and for the outcome variables in the *pre*-publication period[21]. In particular, I divide the assignment variable, i.e. score, into bins and for each variable I plot its average weighted by the number of observations in the bin against the midpoint of each bin. I also add a quadratic fit with confidence bands for the raw data (allowed to be different on the two sides of the cut-off) to aid visualisation of the shape of the underlying function. These plots can usually help to identify differences in the firms at the cut-off but in this context their informative power is limited by the extremely small size of the sample, especially above the cut-off. Figures 3.9, 3.10 and 3.11 in the Appendix show that the cost and value of production[22] and the turnover are very similar across the cut-off. The confidence bands are quite large especially for treated firms: the small sample size of course contributes to a decrease in the precision of the fit and graphs should be interpreted with this caveat in mind.

Finally, figure 3.4 shows the same plots for the outcomes. For patents, the plot shows the number of applications filed up to the end of 2010. For intangibles' intensity, the plot shows the average over the 2007-2010 period[23]. For patents, the

[20] See table 3.6 in the Appendix for a replication of the final ranking.

[21] Similar results are obtained by placebo estimations that are reported in the results section

[22] These two variables represent the main entries in the Income statement (on the revenues and on the cost side). Graphs for other variables look similar but are not reported for the sake of brevity. They are available upon request.

[23] The graphs look identical when restricting to the balanced sample.

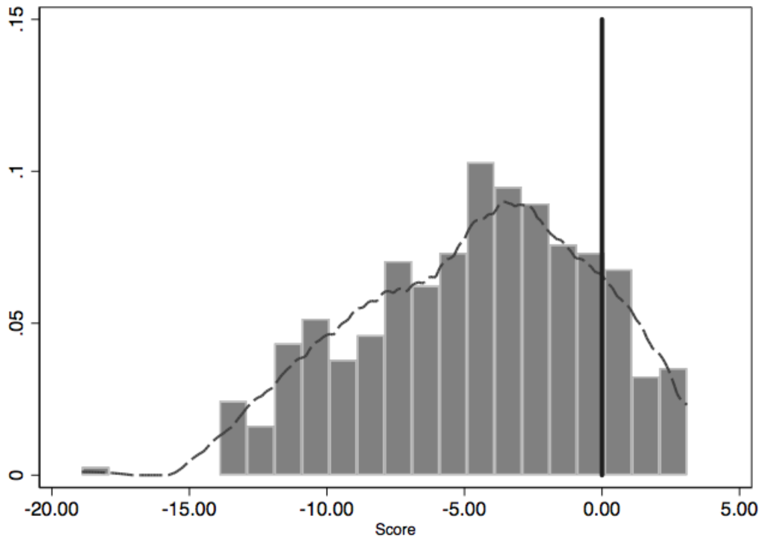


Figure 3.3: **Density distribution of the running variable** The graph also shows the plot of kernel density estimates.

graph is not very informative as most of the observations are around 0. In any case, there does not seem to be any jump above the cut-off. For intangibles' intensity instead, observations above the cut-off seem to have lower intangibles' intensity values on average. Figure 3.12 in the Appendix shows that this difference seems to be driven by the years 2008 and 2009 and to peter out in 2010. In addition, placebo estimations suggest that the difference is not significant. Still, this potential discontinuity must be borne in mind when interpreting the results as it might suggest that the economic criterion of picking cheaper projects has resulted in the selection of worse firms. On the other hand, as other balance sheet entries and patent applications are balanced across the threshold, this result might just derive from the inaccuracy of this metric.

3.5.2 Empirical model

To study the effect of the subsidy on patenting and investment in intangibles I first pool the observations before and after 2010[24] and estimate the treatment effect in

[24] I assume that as the ranking was published in April 2011, 2011 is potentially a post-treatment period.

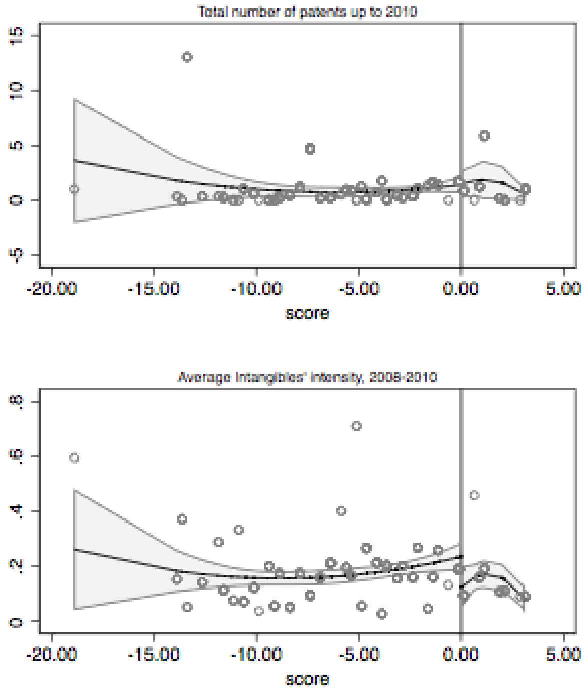


Figure 3.4: **RD plots for outcome variables in the pre-treatment period** These graphs show the average of the variable within bins of score in the the pre-treatment period. The number of patents is cumulated while for intangibles' intensity the graph shows the mean over four years before the treatment. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

these two time periods. Then, I carry an event-study like analysis by looking at how the treatment effect changes year by year[25]. Finally, I look at whether the *change* between the pre and the post period is different around the cut-off, to account for time-invariant unobservables.

To estimate the treatment effect I employ both a parametric and a non-parametric approach. In particular, I first estimate the following simple cross-sectional parametric model on the whole sample in each time period:

$$Y_i = \alpha + \beta(Treated) + \sum_{j=1}^{p} \gamma_j (Score_i^j) + \sum_{j=1}^{p} \delta_j (Score_i^j * Treated_i) + \varepsilon_i \qquad (3.1)$$

[25]As anticipated in section 3.4, my data includes balance sheet up to 2016.

In equation (3.1) Y is the outcome variable, $Treated$ is a dummy treatment variable that is equal to one when the firm wins the grant and zero otherwise, and $Score$ is the normalised running variable with the cut-off centred at 0. The interaction terms allow the relationship between the outcome variable and the running one to be different on the two sides of the discontinuity. At the cut-off, β measures the treatment effect. For observations to the right of the cut-off score the additional effect is given by $(\gamma+\delta)$.

Higher order terms for the running variable and their interactions with the treatment dummy are included in the regression to allow for non-linearities between the outcome and the score. A way to determine the optimal polynomial order is to compute in each time period and for each outcome the Akaike Information Criterion (AIC) for equation (3.1) letting p vary (up to 5). The model with the lowest AIC should have the most preferable "bias-precision" trade-off. However, this criterion delivers only relative comparisons between higher and lower order polynomials only. Another way to test the different models against the data is to check for the joint significance of a set of bin dummies added to equation (3.1) (Lee and Lemieux, 2010). The idea is that if these dummies are jointly significant the functional form is failing to capture some patterns in the data and one has to increase the polynomial order until failing to reject the null. Table 3.7 in the Appendix shows the results of both procedures for intangibles' intensity in the 2008-2012 period. Remarkably, the bin dummies test and AIC lead to quite different results. Detecting the best polynomial order is quite hard given the small number of observations and the high variance in the treatment group. Recently, Gelman and Imbens (2018) have shown that estimates obtained with lower order polynomials are more robust. In this paper, I present results across different polynomial orders as a robustness test. In a RDD context, the treatment status is, by construction, independent of control variables. Yet, to reduce sampling variability in the estimates and to check for the robustness of the results, I also estimate (3.1) including a set of time invariant covariates such as size, sector of activity, administrative region and whether or not the project was presented jointly with a research institute.

For non-parametric estimations, I refer to the most recent literature and estimate a local linear regression around the cut-off (Imbens and Lemieux, 2008; Lee and Lemieux, 2010; Calonico, Cattaneo, and Titiunik, 2014). In particular, I run a local linear kernel regression with triangular weighting, that assigns more weight to the observations closer to the cut-off[26]. An important choice for non-parametric regressions is how many observations around the cut-off should be included. The bandwidth should be large enough to yield more precise estimates but narrow enough to yield less biased estimates. Because of the small number of observations, I use the optimal bandwidth proposed by Imbens and Kalyanaraman (2012) that is larger than the ones proposed by Calonico, Cattaneo, and Titiunik (2014) and Calonico et al. (2018).

On a final note, most of the results are derived from the unbalanced panel. This is because of two main reasons. First, as described in section 3.4, the balanced sample is almost half the size of the unbalanced one, and this can affect the statistical power of the empirical analysis. In RD settings in particular, the number of observations or clusters needed to detect a minimum effect is substantially larger than the one needed under a randomised control trial (Schochet, 2009). Secondly, firms for which data is of good quality in every year are also those that survive more and tend to be larger and perform better than the others, as shown in table 3.2. These factors can all be outcomes of treatment, thus estimations on the balanced panel can be exposed to selection bias. Noticeably, the results from the unbalanced panel can also be biased if missing values are not randomly determined. To quantify this possibility I estimate (3.1) in each year using a dummy for missing value as the outcome variable. The results are reported in table 3.8 in the Appendix and show that there is no statistically significant correlation between the treatment dummy and the quality of data of a firm in a given year.

[26] The difference with respect to a parametric regression around the cut-off is basically the weighting that in the parametric regression is rectangular.

3.6 Results

In this section, I show the main results from the graphical and statistical analyses described in section 3.5. All findings are presented in comparison to the *placebo* results for outcomes determined in the pre-treatment period before the final ranking for the allocation of the subsidy was published. Figure 3.5 shows the RD plots for both outcomes in the post-treatment period. A comparison between figure 3.4 and figure 3.5 seems to suggest that while the patenting activity has not changed differentially for treated and untreated firms after the treatment, the same is not true for intangibles' intensity. In fact, the intensity of intangibles just above the cut-off is on average lower than just below it *before* the publication of the ranking, but higher thereafter. However, as the sample size is very small, especially above the cut-off, the graphical analysis does not allow the drawing of definite conclusions.

Tables 3.3 provides the estimation counterpart of the RD plots. It shows the OLS estimation of β in (3.1) for the two outcomes in the pre and post-treatment periods, for a linear and a quadratic polynomial function of the running variable and with and without controls. Panel A reports the results for the average intensity of intangibles, while Panel B shows the results for the total number of patent applications. The coefficients are unstable across different polynomial orders and specifications. In general, however, there seems to be no statistically significant effect of the treatment on any of the two outcome variables, even if the coefficients of intangibles' intensity are consistently switching sign *after* the treatment[27]. The binary model for patent applications also gives null results[28].

To exploit the time-variation in the post-treatment period, I construct an event-study analysis and estimate equation (3.1) in each year. Figure 3.6 shows the coefficients from these regressions by 4 different polynomial orders. Strikingly, the coefficients are negative and almost significant, depending on the polynomial order, in 2008 and 2009 before the submission system was opened to applications. This

[27]Figures 3.13 and 3.14 in the Appendix show how coefficients stay insignificant up to the 6th oder polynomial.

[28]These are not reported for the sake of space but available upon request.

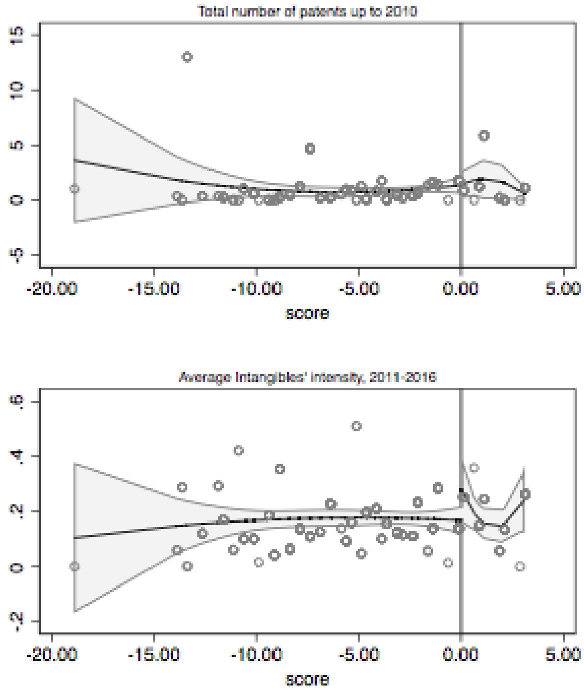


Figure 3.5: **RD plots for outcome variables in the post-treatment period** These graphs show the average of the variable within bins of score in the the post-treatment period. The number of patents is measured on the 2011-2017 period while for intangibles' intensity the graph shows the mean between 2011 and 2016. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

can be problematic as it points to statistically significant differences between treated and untreated firms *before* the treatment. In particular, the negative coefficients suggest that the grant might have been allocated to firms with fewer R&D activities. However, this does not seem plausible, given the context described in section 3.3: one of the criteria for higher scores was a high level of R&D expenditures. Another possible interpretation is that this reflects the rule according to which the cheapest projects having attained the same scores would be preferred, as firms with low R&D activity are plausibly more likely to present less ambitious R&D projects.

Bearing these caveats in mind, the year-by-year analysis identifies a positive and significant effect of the subsidy in 2012 and 2013. This effect is however present in the short-run only. Given the nature of the refunding scheme, a natural interpretation of these results is that the refund for the investments enters the balance sheet in the form of intangible asset as soon as the firm receives it. However, it later decreases because of capital depreciation[29]. In terms of magnitude, the coefficients suggest that firms that received the R&D subsidy increased their intangibles-to-total assets ratio by almost 20% in the immediate years following the publication of the ranking. To test whether these results are driven by the stylised fact that most of R&D subsidies translates into higher personnel cost, I do a similar event study using the log of total wages paid as an outcome and do not find any significant result[30].

To further address the robustness of these results, I estimate (3.1) using the first difference of the outcomes as dependent variables, first between pre and post treatment periods and then as an event study analysis. Figure 3.7 shows that, when the outcome is the change between the average intangibles' intensity before and after the treatment, the coefficients are positive and statistically significant across most polynomial orders. As before, there is an increase of 20% in the *change* in intangibles' intensity after the publication of the ranking for treated firms. On the other hand, and consistently with the year-by-year analysis, the changes in number of total patents filed is unaffected by the publication of the ranking.

To test whether these results are driven by the non-randomness of the missing values, I replicate the analysis on the firms for which I can observe intangibles' intensity in each year. Figure 3.16 in the Appendix shows that the coefficients for $\Delta IntangiblesIntensity$ are still significant and positive. However, the coefficients are larger than in the unbalanced panel; this is reconcilable with the fact that the balanced sample is a selective set of better-performing firms, and this can bias the OLS estimate upward. Finally, these results are also very similar when the dependent variable is the first difference between the outcome measured in any post-treatment year and the outcome measured in 2009. Table 3.4 shows that in fact the coefficient for intangibles' intensity is consistently positive and the largest

[29]These results are not the same in the balanced panel as figure 3.17 in the Appendix shows.

[30]See figure 3.15 in the Appendix. Unfortunately, I could not access wages data after 2012 and so this analysis is restricted to the 2008-2012 period.

effect is detected in 2012[31].

As described in section 3.5, I replicate the analysis by employing non-parametric methods to estimate the treatment effect. Table 3.10 shows that for intangibles' intensity the results are consistent with the ones from parametric regressions. There is a positive and significant increase in the change in average intangibles' intensity in treated firms after the treatment. However, the results for patent applications are not exactly the same as the parametric regressions. In fact, the total number of patent applications is almost 50% lower amongst treated firms after the treatment. This result is driven by the fact that the outcome is equal to 0 for almost 90% of the firms after the treatment and that there are very few firms with many patents. In fact, I obtain similar results when looking at the extensive margin, i.e.a binary variable for patent applications. Also, the results are always insignificant in the year-by-year analysis[32]. Overall then, even if the parametric estimations do not detect any treatment effect on patenting, the non-parametric ones by focusing on the observations around the cut-off find a *negative* effect of the treatment on the probability of patenting. All the other results of non-parametric estimations are very similar to the ones presented and can be found in the Appendix.

3.7 Conclusion

The effectiveness of R&D public support on private firms is often ambiguous. For this reason, governments are increasingly adopting less direct measures such as R&D tax credit (OECD, 2017). However, young firms are believed to face higher credit constraints and fixed costs and thus might still benefit from more direct types of support. In this paper I study the impact of an R&D subsidy to high-tech start-ups introduced by the Italian Government in 2009.

The features of the programme allow the implementation of an RDD as projects were funded on the basis of a score assigned by an independent committee.

[31]These results are robust to the exclusion of controls, specifications with higher polynomial orders and restriction to the balanced sample.

[32]Both results are available upon request.

Table 3.3: Baseline results I : pre and post

Panel A: Average intangibles' intensity

	2008-2010				2011-16			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	-0.105*	-0.022	-0.041	-0.076	0.105	0.015	0.009	0.133
	(0.055)	(0.046)	(0.047)	(0.061)	(0.080)	(0.058)	(0.064)	(0.088)
Observations	350	350	350	350	322	322	322	322
R-squared	0.021	0.012	0.139	0.143	0.011	0.002	0.180	0.195
Polynomial Order	1	2	1	2	1	2	1	2
Controls	NO	NO	YES	YES	NO	NO	YES	YES

Panel B: Total number of patent applications

	up to 2010				2011-17			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	0.075	1.211	1.158	-0.034	-0.233	0.066	0.100	-0.175
	(0.911)	(1.176)	(1.044)	(0.917)	(0.190)	(0.154)	(0.178)	(0.225)
Observations	352	352	352	352	352	352	352	352
R-squared	0.010	0.004	0.232	0.239	0.010	0.007	0.188	0.191
Polynomial Order	1	2	1	2	1	2	1	2
Controls	NO	NO	YES	YES	NO	NO	YES	YES

Notes: the table reports estimates of the treatment effect from parametric regressions of linear and quadratic polynomial order. The dependent variables are the average intangibles' intensity (panel A) and the total number patents filed (panel B) measured over each time period. The samples are smaller than in the text because of missing values for the industry control. The regressions include the running variable and its interaction with the treatment, as in equation (3.1). The controls used in columns 3-4 and 6-7 are: 4-digit NACE industry dummies, size dummies and a dummy for firms in convergence regions. The unit of observation is a firm. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

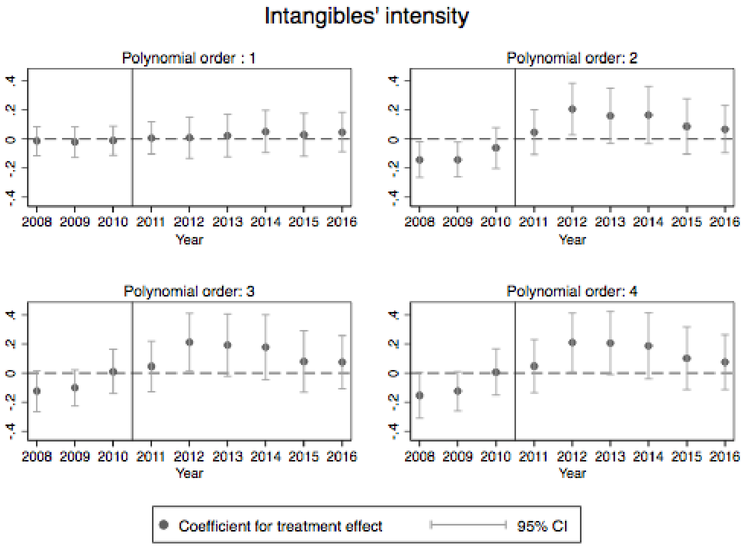


Figure 3.6: **Intangibles' intensity. Treatment effect by year and polynomial order.** These graphs plot the coefficients of the treatment effect in each year before and after the publication of the ranking. Each chart shows the results for different polynomial orders of the running variable and its interaction with the treatment dummy. The unit of observation is a firm.

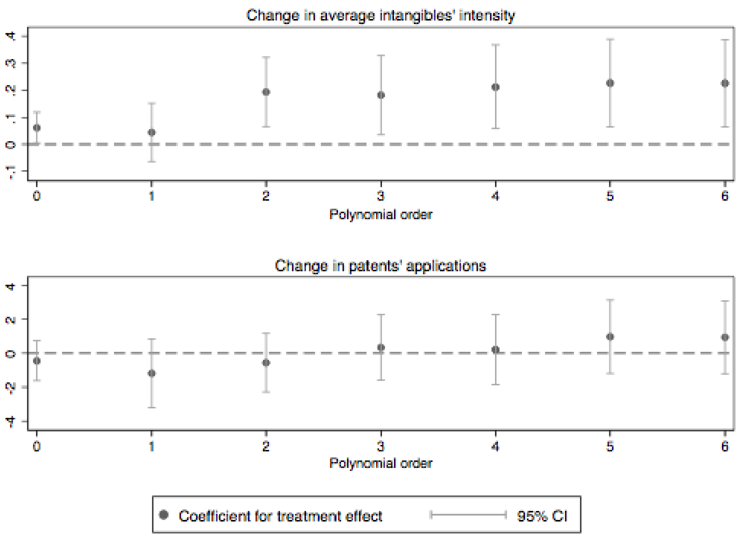


Figure 3.7: **Treatment effect by polynomial order.** These graphs plot the coefficients of the treatment effect of the *changes* in outcome after the publication of the ranking. The coefficients are plotted against the polynomial order of the function of the running variable and its interaction with the treatment dummy. The top chart shows the changes in intangibles' intensity while the bottom one shows growth rate in the number of patent applications. The unit of observation is a firm.

Table 3.4: Differences in discontinuity: year by year

	Panel A: Δ Intangibles' intensity					
Δ between	2009-2011	2009-2012	2009-2013	2009-2014	2009-2015	2009-2016
	(1)	(2)	(3)	(4)	(5)	(6)
treated	0.194***	0.316***	0.228**	0.295***	0.240*	0.284***
	(0.072)	(0.080)	(0.093)	(0.093)	(0.126)	(0.099)
Observations	289	254	276	260	237	212
R-squared	0.131	0.195	0.165	0.153	0.147	0.190
Polynomial Order	2	2	2	2	2	2
Controls	YES	YES	YES	YES	YES	YES
	Panel B: Δ number of patent applications					
Δ between	2009-2011	2009-2012	2009-2013	2009-2014	2009-2015	2009-2016
	(1)	(2)	(3)	(4)	(5)	(6)
treated	0.054	-0.003	0.092	0.141*	0.022	0.107
	(0.152)	(0.143)	(0.108)	(0.082)	(0.078)	(0.092)
Observations	334	334	334	334	334	334
R-squared	0.286	0.147	0.142	0.088	0.235	0.191
Polynomial Order	2	2	2	2	2	2
Controls	YES	YES	YES	YES	YES	YES

Notes: the table reports estimates of the treatment effect from parametric regressions quadratic polynomial order. The dependent variables are differences between intangibles' intensity (panel A) and the total number patents filed (panel B) in each post-treatment year and the variables in 2009. The regressions include the running variable and its interaction with the treatment, as in equation (3.1). The controls used in columns 3-4 and 6-7 are: 4-digit NACE industry dummies, size dummies and a dummy for firms in convergence regions. The unit of observation is a firm. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 3.5: Non-parametric estimation: differences in discontinuity

	Panel A: Average intangibles' intensity		
	Levels		Δ
	Pre-2011	Post-2011	Post - Pre
	(1)	(2)	(3)
treated	-0.0647	0.112	0.211**
	(0.0589)	(0.0835)	(0.0826)
Observations	358	329	326
BW size	2.577	2.174	1.833
	Panel B: Total patent applications		
	Levels		Δ
	Pre-2011	Post-2011	Post - Pre
	(1)	(2)	(3)
treated	-0.188	-0.474**	-0.324
	(1.121)	(0.222)	(1.039)
Observations	361	361	361
BW size	3.439	2.835	4.242

Notes: the table reports estimates of the treatment effect from non-parametric regressions. Panel A is for intangibles' intensity and panel B for patents' application. The outcome variables in columns (1) and (2) are measured in pre and post-treatment period, respectively. Column (3) reports the coefficient for the first difference. The treatment occurs in 2011.The optimal bandwidth is the one suggested by Imbens and Kalyanaraman (2012) . The unit of observation is a firm. Standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Most importantly, the cut-off score was unknown ex-ante and this helps to rule out non-random sorting around it. Using balance sheet and patenting data, I find that firms which received the subsidy have a 20% higher intangibles-to-assets ratio than firms that did not. The effect is significant only in the two years following the publication of the ranking and fades away later on. I find no robust evidence for an effect on patents but non-parametric regressions suggest that treated firms have patented less in the years following the subsidy. Finally, I find no effect on other balance sheet entries, such as sales.

The results suggest that a large and positive effect on intangibles' intensity. As start-ups are more credit-constrained and also less able to secure innovative results, this effect can be interpreted as an upper bound for larger and more mature firms. These findings are consistent with papers that find positive effects on smaller firms only (Lach, 2002; Bronzini and Piselli, 2016). However, the results on patenting and other key metrics of firms performance are null. A potential interpretation of these results is that the impact on the intensity of intangibles rather than reflecting an increase in innovative activities might be the mechanic accounting response to the increase in capital derived from the reception of the funds. In terms of innovation outputs, treated firms do not patent more and, at worst, they patent less. In other words, even if the subsidy allowed a one-shot increase in R&D investments, this did not have positive externalities on firms' performance or on firms' innovative activity in general. These findings would be consistent with previous studies that focus on the features of innovations that are strategically made outside the patent system (see, for instance, Moser, 2005). Also, the subsidy was targeted mainly at the industrial development of products. Development projects are less risky, less uncertain, less intangible and also less likely to lead to patenting (Griliches, 1986).

The results of this paper have some limitations. First, as already mentioned, RDD requires large sample sizes to detect the local effect of the treatment. Therefore, the statistical power of this analysis is probably too low due to the very small number of firms applying for the grant in the first place. This also precludes heterogeneity analyses. Secondly, data limitations make it very hard to identify actual innovation. Finally, the timing of the programme might have created some distortions that this work could not detect. The publication of the ranking was supposed to occur on April 2010 but was postponed by one year until April 2011. It is very plausible to suppose that young firms with innovative projects in mind might have just started the investments anyway before the publication of the ranking. If subsidised, they might have directed the public funds to additional projects or other non-innovation related costs. Because of data limitations, I cannot identify the actual R&D investments of the firms and thus I cannot rule out that the subsidies went to infra-marginal projects, that would have been carried out irrespective of the subsidy.

3.A Appendix

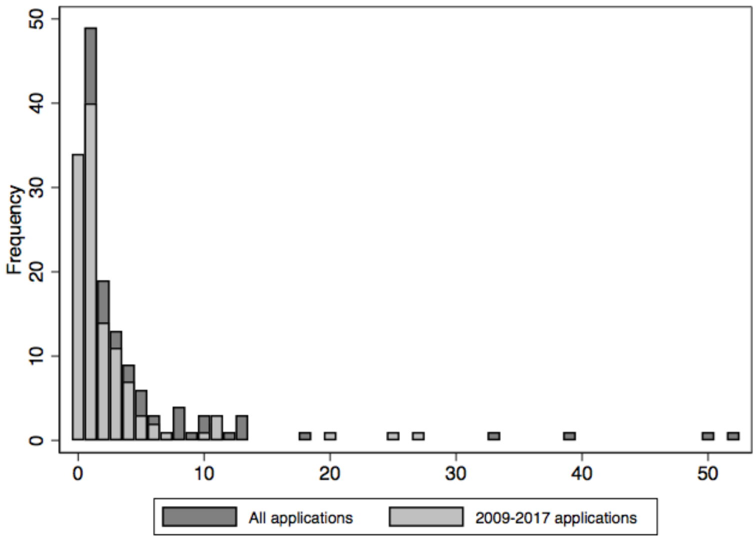


Figure 3.8: **Distribution of patent applications** The sample is restricted to firms that applied at least once for a patent.

Table 3.6: Ranking around the cut-off

Ranking	Score	Grant
52	18.9	YES
53	18.9	YES
54	18.9	YES
55	18.9	YES
56	18.9	YES
57	18.9	YES
58	18.9	YES
59	18.9	NO
60	18.9	NO
61	18.9	NO
62	18.9	NO

Notes: the table shows the score distribution around the cut-off.

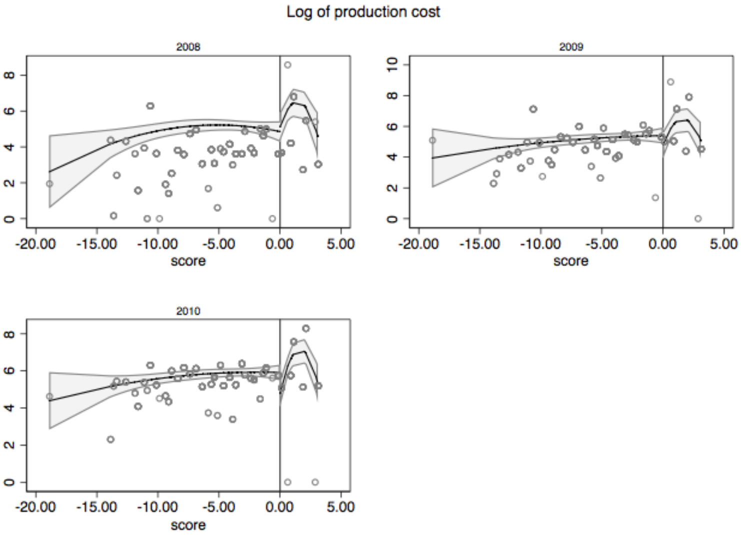


Figure 3.9: **RD plots for production cost in the pre-treatment period** These graphs show the average of the variable within bins of score in each year of the pre-treatment period. The variable is transformed as log(1+x). The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

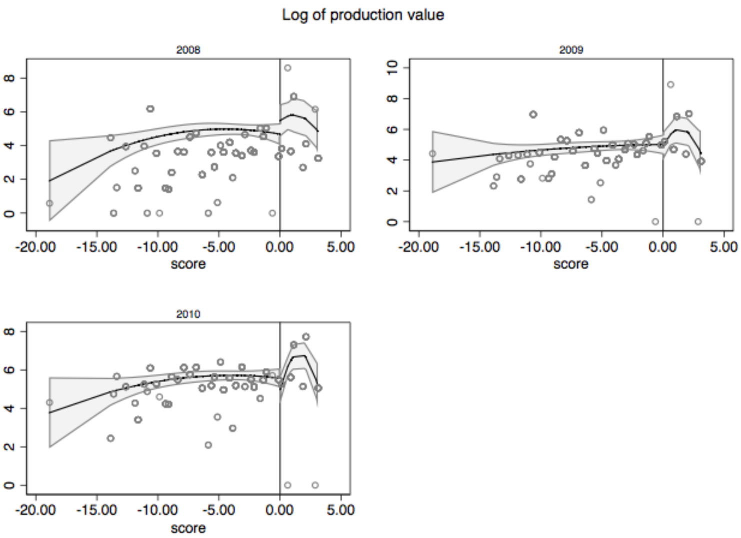


Figure 3.10: **RD plots for production value in the pre-treatment period** These graphs show the average of the variable within bins of score in each year of the pre-treatment period. The variable is transformed as log(1+x). The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

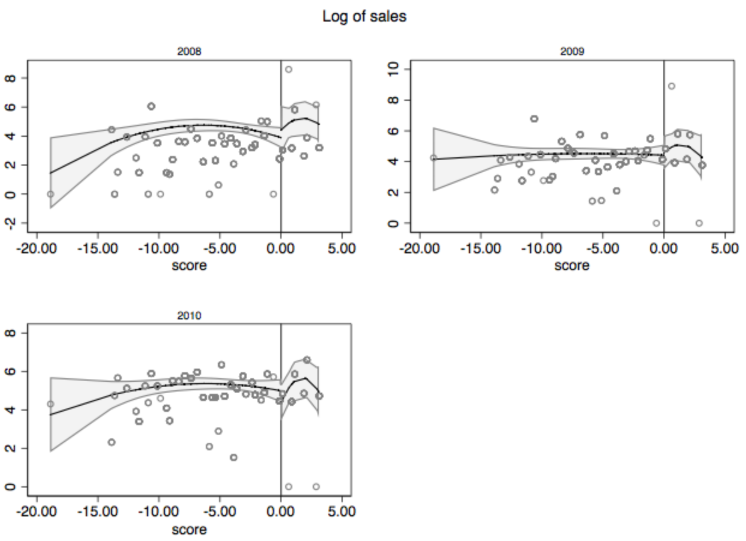


Figure 3.11: **RD plots for sales in the pre-treatment period** These graphs show the average of the variable within bins of score in each year of the pre-treatment period. The variable is transformed as log(1+x).The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

Table 3.7: Optimal polynomial order

	Selection criterion				
	AIC				**Bin dummies**
	All	Treatment	Control	max	
2008	2	2	2	2	1
2009	3	3	3	3	5
2010	3	1	3	3	5
2011	1	1	1	1	1

Notes: the table reports the optimal polynomial order obtained in each year by the AIC and the bin dummies test suggested by Lee and Lemieux (2010). The polynomial orders selected by AIC are computed on the whole sample, on the treatment group sample, and on the control group sample. The last column reports the maximum among those three.

Table 3.8: Balance sheet missing values

	Dummy for no missing value							
	Pre		Post					
year	2009	2010	2011	2012	2013	2014	2015	2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	0.0234	-0.0305	0.0759	-0.0619	-0.0602	-0.00530	0.0747	0.130
	(0.0597)	(0.0571)	(0.0927)	(0.114)	(0.102)	(0.111)	(0.120)	(0.127)
Observations	361	361	361	361	361	361	361	361
R-squared	0.017	0.004	0.009	0.048	0.020	0.017	0.018	0.009
Polynomial Order	1	1	1	1	1	1	1	1
Controls	NO	NO	NO	NO	NO	NO	NO	NO

Notes: the table reports estimates of the treatment effect from linear regressions. The dependent variable is a dummy equal to one if core balance sheet data are not available for the firm in that year. The regressions include the running variable and its interaction with the treatment, as in equation (3.1). The unit of observation is a firm. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 3.9: Baseline results: pre and post - Balanced panel

	Panel A: Intangibles' intensity							
	2008-2010				2011-16			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	-0.152***	-0.082*	-0.043	-0.109**	0.130	0.041	0.066	0.167*
	(0.048)	(0.046)	(0.046)	(0.053)	(0.095)	(0.074)	(0.076)	(0.098)
Observations	181	181	181	181	181	181	181	181
R-squared	0.060	0.049	0.180	0.187	0.032	0.018	0.073	0.088
Polynomial Order	1	2	1	2	1	2	1	2
Controls	NO	NO	YES	YES	NO	NO	YES	YES
	Panel B: Total number of patent applications							
	up to 2010				2011-17			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	0.157	0.824	1.000	0.451	-0.306	-0.017	0.106	-0.270
	(1.413)	(1.005)	(1.077)	(2.069)	(0.341)	(0.243)	(0.218)	(0.426)
Observations	181	181	181	181	181	181	181	181
R-squared	0.025	0.010	0.034	0.051	0.025	0.021	0.116	0.122
Polynomial Order	1	2	1	2	1	2	1	2
Controls	NO	NO	YES	YES	NO	NO	YES	YES

Notes: the table reports estimates of the treatment effect from parametric regressions of linear and quadratic polynomial order. The dependent variables are the average intangibles' intensity (panel A) and the total number patents filed (panel B) measured over each time period. The samples are created by restricting to firms with no missing values in any of the years. The regressions include the running variable and its interaction with the treatment, as in equation (3.1). The controls used in columns 3-4 and 6-7 are: 4-digit NACE industry dummies, size dummies and a dummy for firms in convergence regions. The unit of observation is a firm. Heteroskedasticity-robust standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 3.10: Non-parametric estimation: difference in discontinuity II

	Δ **Intangibles' intensity**		Δ **Patents' applications**	
	(1)	(2)	(3)	(4)
treated	0.211**	0.341***	-0.277	-0.401
	(0.0826)	(0.105)	(1.037)	(0.992)
Observations	320	181	352	181
Balanced panel	NO	YES	NO	YES
BW size	1.832	1.988	3.906	3.849

Notes: the table reports estimates of the treatment effect from non-parametric regressions. The outcome variables are the changes in intangibles' intensity and patents' application between the pre- and post-treatment period. The treatment occurs in 2011.The optimal bandwidth is the one suggested by Imbens and Kalyanaraman (2012) . The unit of observation is a firm. Standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 3.11: Non-parametric estimation: year by year I

	Intangibles' intensity							
	Panel A: Unbalanced panel							
year	2009	2010	2011	2012	2013	2014	2015	2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	-0.113	-0.0121	0.0282	0.221**	0.199**	0.201*	0.106	0.0740
	(0.0738)	(0.0794)	(0.0920)	(0.0901)	(0.0962)	(0.103)	(0.101)	(0.0895)
Observations	341	343	308	264	292	275	250	224
BW size	2.225	2.225	2.225	2.225	2.225	2.225	2.225	2.225
	Panel B: Balanced panel							
year	2009	2010	2011	2012	2013	2014	2015	2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	-0.274***	-0.0398	0.0357	0.184*	0.153	0.138	0.183	0.112
	(0.0934)	(0.0978)	(0.118)	(0.100)	(0.117)	(0.122)	(0.125)	(0.0999)
Observations	183	183	183	183	183	183	183	183
BW size	2.218	2.218	2.218	2.218	2.218	2.218	2.218	2.218

Notes: the table reports estimates of the treatment effect from non-parametric regressions in each year. The outcome variable is intangibles' intensity. The treatment occurs in 2011.The optimal bandwidth is computed in 2009 and is the one suggested by Imbens and Kalyanaraman (2012) . Panel A reports results on the unbalanced sample. Panel B reports results on the balanced sample. The unit of observation is a firm. Standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

Table 3.12: Non-parametric estimation: year by year II

	Patents' applications							
	Panel A: Unbalanced panel							
year	2009	2010	2011	2012	2013	2014	2015	2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	-0.493	-0.357	-0.0756	-0.180*	-0.101	0.0300	-0.0349	-0.113
	(0.379)	(0.263)	(0.0772)	(0.106)	(0.114)	(0.0301)	(0.0271)	(0.0882)
Observations	361	361	361	361	361	361	361	361
BW size	2.850	2.850	2.850	2.850	2.850	2.850	2.850	2.850
	Panel B: Balanced panel							
year	2009	2010	2011	2012	2013	2014	2015	2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treated	-0.540	-0.0458	-0.151	-0.0932	-0.252	0.0386	-0.0233	-0.105
	(0.592)	(0.169)	(0.114)	(0.152)	(0.179)	(0.0566)	(0.0258)	(0.105)
Observations	183	183	183	183	183	183	183	183
BW size	3.505	3.505	3.505	3.505	3.505	3.505	3.505	3.505

Notes: the table reports estimates of the treatment effect from non-parametric regressions in each year. The outcome variable is the number of patent applications filed in each year. The treatment occurs in 2011.The optimal bandwidth is computed in 2009 and is the one suggested by Imbens and Lemieux (2008) . Panel A reports results on the unbalanced sample. Panel B reports results on the balanced sample. The unit of observation is a firm. Standard errors in parenthesis. *** significant at less than 1 percent; ** significant at 5 percent; * significant at 10 percent.

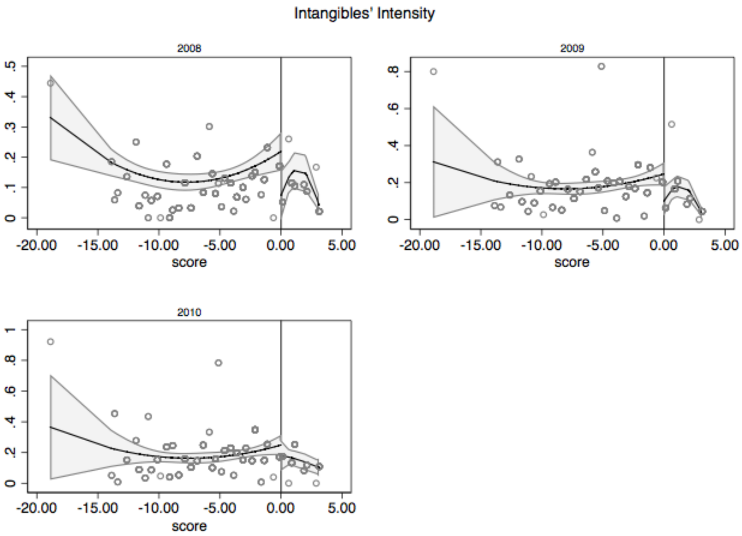


Figure 3.12: **RD plots** These graphs show the average of the variable within bins of score in each year of the pre-treatment period. The circles represent means within 10% bins and the lines and shaded areas are fitted values and 90% confidence intervals based on a quadratic polynomial regression on each side of the cut-off.

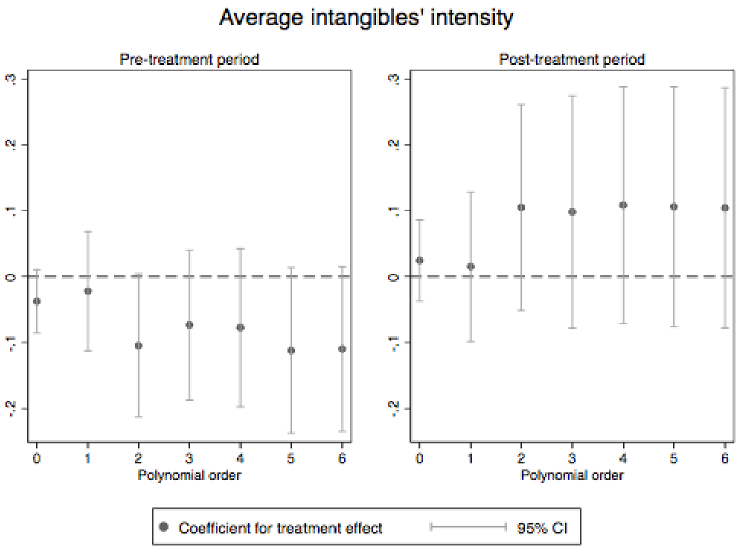


Figure 3.13: **Intangibles' intensity. Treatment effect by polynomial order** These graphs plot the coefficients of the treatment effect in the pre (2008-2010) and post (2011-2016) period, against polynomial orders of the running variable. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a firm.

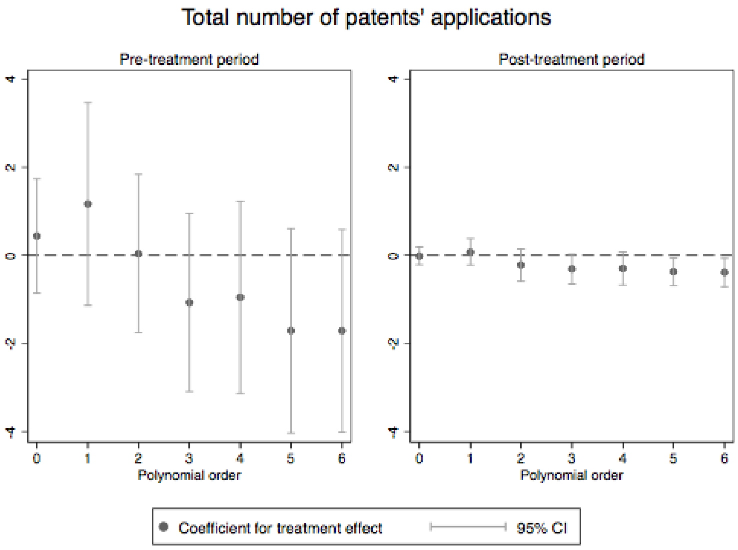


Figure 3.14: **Total number of patent applications. Treatment effect by polynomial order** These graphs plot the coefficients of the treatment effect in the pre and post period, against polynomial orders of the running variable. The running variable is interacted with the treatment dummy to allow for different polynomials on each side of the cut-off. The unit of observation is a firm.

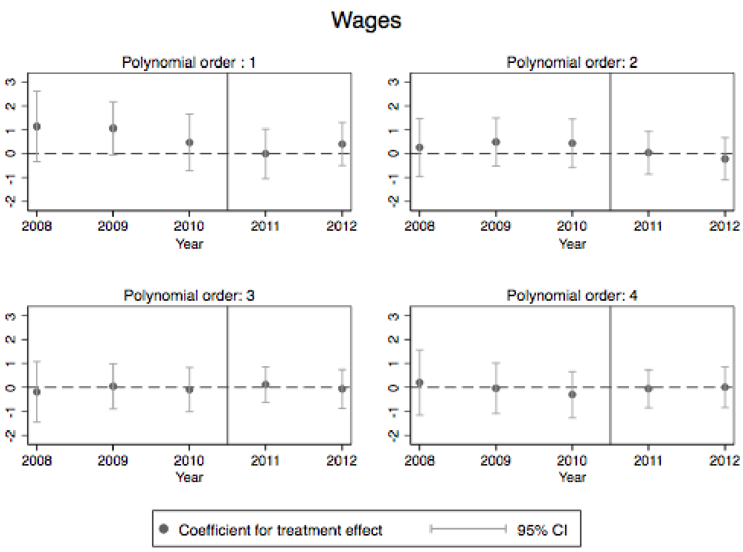


Figure 3.15: **Log of wages. Treatment effect by year and polynomial order.** These graphs plot the coefficients of the treatment effect in each year before and after the publication of the ranking (only up to 2012 because of data limitation). Each chart shows the results for different polynomial orders of the running variable and its interaction with the treatment dummy. The unit of observation is a firm.

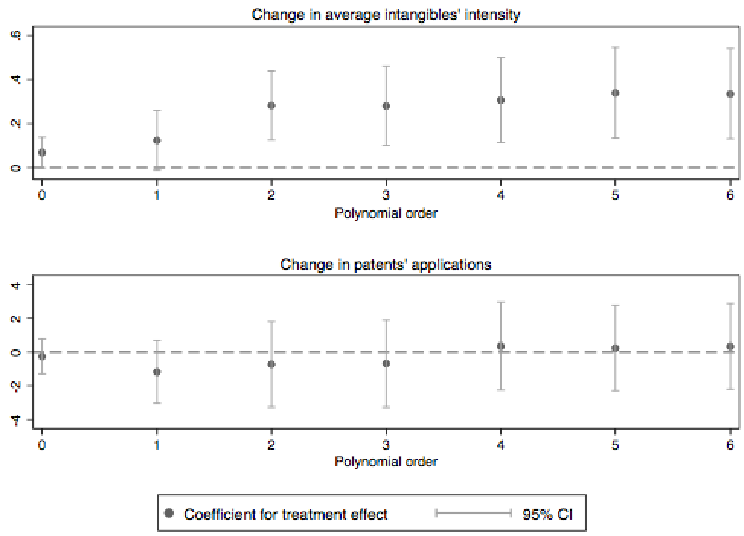


Figure 3.16: **Treatment effect by polynomial order - Balanced panel.** These graphs plot the coefficients of the treatment effect of the *changes* in outcome after the publication of the ranking. The coefficients are plotted against the polynomial order of the function of the running variable and its interaction with the treatment dummy. The top chart shows the changes in intangibles' intensity while the bottom one shows growth rate in the number of patent applications. The sample is restricted to the balanced panel. The unit of observation is a firm.

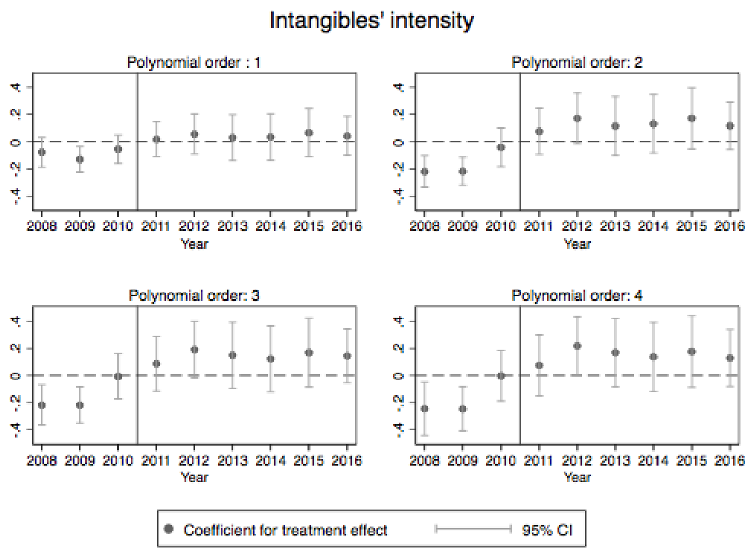


Figure 3.17: **Intangibles' intensity. Treatment effect by year and polynomial order - Balanced panel** These graphs plot the coefficients of the treatment effect in each year before and after the publication of the ranking. Each chart shows the results for different polynomial orders of the running variable and its interaction with the treatment dummy. The sample is the balanced panel. The unit of observation is a firm.

Bibliography

Albouy, David. 2013. "Partisan representation in Congress and the geographic distribution of Federal funds." *Review of Economics and Statistics* 95 (1):127–141.

Alesina, Alberto, Filipe R Campante, and Guido Tabellini. 2008. "Why is fiscal policy often procyclical?" *Journal of the european economic association* 6 (5):1006–1036.

Almus, Matthias and Dirk Czarnitzki. 2003. "The effects of public R&D subsidies on firms' innovation activities: the case of Eastern Germany." *Journal of Business & Economic Statistics* 21 (2):226–236.

Arrow, Kenneth. 1962. "Economic welfare and the allocation of resources for innovations. Nelson ed., The rate and direction of incentive activity: Economic and social factors."

Arulampalam, Wiji, Sugato Dasgupta, Amrita Dhillon, and Bhaskar Dutta. 2009. "Electoral goals and center-state transfers: A theoretical model and empirical evidence from India." *Journal of Development Economics* 88 (1):103 – 119.

Austin, Benjamin A, Edward L Glaeser, and Lawrence H Summers. 2018. "Jobs for the Heartland: Place-Based Policies in 21st Century America." Working Paper 24548, National Bureau of Economic Research.

Baffigi, Alberto. 2011. "Italian National Accounts, 1861-2011." *Bank of Italy - Economic History Working Papers* (18).

Barreca, Alan I, Melanie Guldi, Jason M Lindo, and Glen R Waddell. 2011. "Saving babies? Revisiting the effect of very low birth weight classification." *The Quarterly Journal of Economics* 126 (4):2117–2123.

Bartik, Timothy J. 1991. "Who benefits from state and local economic development policies?" .

———. 2014. "How effects of local labor demand shocks vary with local labor market conditions." .

Barucci, Piero. 1978. *Ricostruzione, pianificazione, Mezzogiorno: la politica economica in Italia dal 1943 al 1955*. Il mulino Bologna.

Baskaran, Thushyanthan and Zohal Hessami. 2017. "Political alignment and intergovernmental transfers in parliamentary systems: Evidence from Germany." *Public Choice* 171 (1-2):75–98.

Becker, Sascha O, Peter H Egger, and Maximilian Von Ehrlich. 2010. "Going NUTS: The effect of EU Structural Funds on regional performance." *Journal of Public Economics* 94 (9):578–590.

Becker, Sascha O., Peter H. Egger, and Maximilian von Ehrlich. 2012. "Too much of a good thing? On the growth effects of the EU's regional policy." *European Economic Review* 56 (4):648 – 668.

Bernardi, Emanuele. 2006. *La riforma agraria in Italia e gli Stati Uniti: Guerra fredda, Piano Marshall e interventi per il Mezzogiorno negli anni del centrismo degasperiano*. Il mulino.

Besley, Timothy and Anne Case. 1995. "Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits." *The Quarterly Journal of Economics* 110 (3):769–798.

Bracco, Emanuele, Ben Lockwood, Francesco Porcelli, and Michela Redoano. 2015. "Intergovernmental grants as signals and the alignment effect: Theory and evidence." *Journal of Public Economics* 123 (Supplement C):78 – 91.

Brollo, Fernanda and Tommaso Nannicini. 2012. "Tying Your Enemy's Hands in Close Races: The Politics of Federal Transfers in Brazil." *American Political Science Review* 106 (4):742 – 761.

Bronzini, Raffaello and Eleonora Iachini. 2014. "Are incentives for R&D effective? Evidence from a regression discontinuity approach." *American Economic Journal: Economic Policy* 6 (4):100–134.

Bronzini, Raffaello and Paolo Piselli. 2016. "The impact of R&D subsidies on firm innovation." *Research Policy* 45 (2):442–457.

Burbidge, John B., Lonnie Magee, and A. Leslie Robb. 1988. "Alternative Transformations to Handle Extreme Values of the Dependent Variable." *Journal of the American Statistical Association* 83 (401):123–127.

Burgess, Robin, Remi Jedwab, Edward Miguel, Ameet Morjaria, and Gerard Padr 'o i Miquel. 2015. "The Value of Democracy: Evidence from Road Building in Kenya." *American Economic Review* 105 (6):1817–51.

Busom, Isabel. 2000. "An empirical evaluation of the effects of R&D subsidies." *Economics of innovation and new technology* 9 (2):111–148.

Busso, Matias, Jesse Gregory, and Patrick Kline. 2013. "Assessing the incidence and efficiency of a prominent place based policy." *American Economic Review* 103 (2):897–947.

Cafiero, Salvatore. 2000. *Storia dell'intervento straordinario nel Mezzogiorno, 1950-1993.* P. Lacaita.

Calonico, Sebastian, Matias D Cattaneo, and Max H Farrell. 2018. "Optimal Bandwidth Choice for Robust Bias Corrected Inference in Regression Discontinuity Designs." Tech. rep.

Calonico, Sebastian, Matias D Cattaneo, Max H Farrell, and Rocio Titiunik. 2018. "Regression discontinuity designs using covariates." *Review of Economics and Statistics, forthcoming* .

Calonico, Sebastian, Matias D Cattaneo, and Rocio Titiunik. 2014. "Robust nonparametric confidence intervals for regression-discontinuity designs." *Econometrica* 82 (6):2295–2326.

Carboni, Oliviero A. 2011. "R&D subsidies and private R&D expenditures: evidence from Italian manufacturing data." *International Review of Applied Economics* 25 (4):419–439.

Carozzi, Felipe and Luca Repetto. 2016. "Sending the pork home: Birth town bias in transfers to Italian municipalities." *Journal of Public Economics* 134 (Supplement C):42 – 52.

Cattaneo, Matiad D, Nicolas Idrobo, and Rocio Titiunik. 2018. "A Practical Introduction to Regression Discontinuity Designs: Part I. Cambridge Elements: Quantitative and Computational Methods for Social Science."

Cattaneo, Matias D, Michael Jansson, and Xinwei Ma. 2017. "Simple local polynomial density estimators." *University of Michigan, Working Paper* .

Caughey, Devin and Jasjeet S Sekhon. 2011. "Elections and the regression discontinuity design: Lessons from close US house races, 1942–2008." *Political Analysis* 19 (4):385–408.

Cerqua, Augusto and Guido Pellegrini. 2014. "Do subsidies to private capital boost firms' growth? A multiple regression discontinuity design approach." *Journal of Public Economics* 109:114–126.

Chubb, Judith. 1982. *Patronage, power and poverty in southern Italy: a tale of two cities*. Cambridge University Press.

Corbetta, Piergiorgio and Maria Serena Piretti. 2009. *Atlante storico-elettorale d'Italia: 1861-2008*. Zanichelli.

Cox, Gary W. and Mathew D. McCubbins. 1986. "Electoral Politics as a Redistributive Game." *The Journal of Politics* 48 (2):370–389.

Crafts, Nicholas and Marco Magnani. 2013. "The Golden Age and the Second Globalization in Italy." In *The Oxford Handbook of the Italian Economy Since Unification.*

d'Adda, Giovanna and Guido de Blasio. 2017. "Historical legacy and policy effectiveness: the long-term influence of pre-unification borders in Italy." *Journal of Regional Science* 57 (2):319–341.

Daniele, Vittorio and Paolo Malanima. 2007. "Il prodotto delle regioni e il divario Nord-Sud in Italia (1861-2004)." *Rivista di politica economica* :267–315.

David, Paul A, Bronwyn H Hall, and Andrew A Toole. 2000. "Is public R&D a complement or substitute for private R&D? A review of the econometric evidence." *Research policy* 29 (4-5):497–529.

Dellmuth, Lisa Maria and Michael F Stoffel. 2012. "Distributive politics and intergovernmental transfers: The local allocation of European Union structural funds." *European Union Politics* 13 (3):413–433.

Dixit, Avinash and John Londregan. 1996. "The Determinants of Success of Special Interests in Redistributive Politics." *The Journal of Politics* 58 (4):1132–1155.

Duranton, Gilles and Anthony J Venables. 2018. *Place-based policies for development.* The World Bank.

Eckaus, Richard S. 1961. "The North-South Differential In Italian Economic Development." *The Journal of Economic History* 21 (03):285–317.

Eggers, Andrew C, Anthony Fowler, Jens Hainmueller, Andrew B Hall, and James M Snyder Jr. 2015. "On the validity of the regression discontinuity design for estimating electoral effects: New evidence from over 40,000 close races." *American Journal of Political Science* 59 (1):259–274.

Einiö, Elias. 2014. "R&D subsidies and company performance: Evidence from geographic variation in government funding based on the ERDF population-density rule." *Review of Economics and Statistics* 96 (4):710–728.

Eisensee, Thomas and David Strömberg. 2007. "News droughts, news floods, and US disaster relief." *The Quarterly Journal of Economics* 122 (2):693–728.

Fauri, Francesca. 2010. *Il piano Marshall e l'Italia*. Il mulino.

Faye, Michael and Paul Niehaus. 2012. "Political aid cycles." *American Economic Review* 102 (7):3516–30.

Federico, Giovanni and Michelangelo Vasta. 2017. "The Origins of the Italian Regional Divide: Evidence from Real Wages, 1861-1913." .

Felice, Emanuele. 2015. *Ascesa e declino: storia economica d'Italia*. Il Mulino.

Felice, Emanuele and Amedeo Lepore. 2017. "State intervention and economic growth in Southern Italy: the rise and fall of the Cassa per il Mezzogiorno(1950–1986)." *Business History* 59 (3):319–341.

Ferreira, Fernando and Joseph Gyourko. 2009. "Do political parties matter? Evidence from US cities." *The Quarterly journal of economics* 124 (1):399–422.

Gelman, Andrew and Guido Imbens. 2018. "Why high-order polynomials should not be used in regression discontinuity designs." *Journal of Business & Economic Statistics* :1–10.

Giorcelli, Michela. 2016. *Economic Recovery and the Determinants of Productivity and Innovation: Evidence from Post-WWII Italy*. Ph.D. thesis, Stanford University.

Glaeser, Edward L and Joshua D Gottlieb. 2008. "The Economics of Place-Making Policies." *Brookings Papers on Economic Activity* (1).

Golden, Miriam A and Lucio Picci. 2008. "Pork-barrel politics in postwar Italy, 1953–94." *American Journal of Political Science* 52 (2):268–289.

Goolsbee, Austan. 1998. "Does government R&D policy mainly benefit scientists and engineers?" Tech. rep., National Bureau of Economic Research.

Griliches, Zvi. 1986. "Productivity, research-and-development, and basic research at the firm level in the 1970s." *American Economic Review* 76 (1):141–154.

Grimmer, Justin, Eitan Hersh, Brian Feinstein, and Daniel Carpenter. 2011. "Are close elections random?" *Unpublished manuscript* .

Grossman, Gene M and Elhanan Helpman. 1996. "Electoral competition and special interest politics." *The Review of Economic Studies* 63 (2):265–286.

Guiso, Luigi. 1998. "High-tech firms and credit rationing." *Journal of Economic Behavior & Organization* 35 (1):39–59.

Hall, Bronwyn. 1996. *Technology, R&D, and the Economy*, chap. The Private and Social Returns to Research and Development: What Have We Learned? The Brookings Institution and the American Enterprise Institute.

Hall, Bronwyn H. 2002. "The financing of research and development." *Oxford review of economic policy* 18 (1):35–51.

Hall, Bronwyn H and Josh Lerner. 2010. "The financing of R&D and innovation." In *Handbook of the Economics of Innovation*, vol. 1. Elsevier, 609–639.

Ham, John C, Charles Swenson, Ayşe İmrohoroğlu, and Heonjae Song. 2011. "Government programs can improve local labor markets: Evidence from state enterprise zones, federal empowerment zones and federal enterprise community." *Journal of Public Economics* 95 (7-8):779–797.

Howell, Sabrina T. 2017. "Financing innovation: evidence from R&D grants." *American Economic Review* 107 (4):1136–64.

Ilzetzki, Ethan. 2011. "Rent-seeking distortions and fiscal procyclicality." *Journal of Development Economics* 96 (1):30 – 46.

Imbens, Guido and Karthik Kalyanaraman. 2012. "Optimal bandwidth choice for the regression discontinuity estimator." *The Review of economic studies* 79 (3):933–959.

Imbens, Guido W and Thomas Lemieux. 2008. "Regression discontinuity designs: A guide to practice." *Journal of econometrics* 142 (2):615–635.

Istat. 1998. *I censimenti delle atitvità produttive dal 1951 al 1991: dati comunali.* SSN.

ISTAT. 2017a. *L'offerta comunale di asili nido e altri servizi socio-educativi per la prima infanzia.* ISTAT.

———. 2017b. *Rilevazione Forze Lavoro.* ISTAT.

Iuzzolino, Giovanni, Guido Pellegrini, and Gianfranco Viesti. 2013. "Regional convergence." In *The Oxford handbook of the Italian economy since unification.*

Jayne, Thomas S, John Strauss, Takashi Yamano, and Daniel Molla. 2002. "Targeting of food aid in rural Ethiopia: chronic need or inertia?" *Journal of Development Economics* 68 (2):247 – 288.

Johansson, Eva. 2003. "Intergovernmental grants as a tactical instrument: empirical evidence from Swedish municipalities." *Journal of Public Economics* 87 (5):883 – 915.

Keefer, Philip. 2007. "Clientelism, credibility, and the policy choices of young democracies." *American journal of political science* 51 (4):804–821.

Kline, Patrick and Enrico Moretti. 2013. "Place based policies with unemployment." *American Economic Review* 103 (3):238–43.

———. 2014a. "Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority." *The Quarterly Journal of Economics* 129 (1):275–331.

———. 2014b. "People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs." *Annual Review of Economics* 6 (1):629–662.

Kuziemko, Ilyana and Eric Werker. 2006. "How much is a seat on the Security Council worth? Foreign aid and bribery at the United Nations." *Journal of political economy* 114 (5):905–930.

Lach, Saul. 2002. "Do R&D subsidies stimulate or displace private R&D? Evidence from Israel." *The journal of industrial economics* 50 (4):369–390.

Larcinese, Valentino, Leonzio Rizzo, and Cecilia Testa. 2006. "Allocating the US federal budget to the states: The impact of the president." *The Journal of Politics* 68 (2):447–456.

Lee, David S and Thomas Lemieux. 2010. "Regression discontinuity designs in economics." *Journal of economic literature* 48 (2):281–355.

Lee, David S, Enrico Moretti, and Matthew J Butler. 2004. "Do voters affect or elect policies? Evidence from the US House." *The Quarterly Journal of Economics* 119 (3):807–859.

Lepore, Amedeo. 2014. "La Cassa per il Mezzogiorno. Dal recupero dell?archivio alla promozione della ricerca."

Lerner, Josh. 2000. "The government as venture capitalist: the long-run impact of the SBIR program." *The Journal of Private Equity* :55–78.

Levitt, Steven D and James M Snyder Jr. 1995. "Political parties and the distribution of federal outlays." *American Journal of Political Science* :958–980.

Lindbeck, Assar and Jrgen W. Weibull. 1987. "Balanced-Budget Redistribution as the Outcome of Political Competition." *Public Choice* 52 (3):273–297.

Lizzeri, Alessandro and Nicola Persico. 2001. "The Provision of Public Goods under Alternative Electoral Incentives." *American Economic Review* 91 (1):225–239.

Mangullo, Stefano. 2015. *Dal fascio allo scudo crociato - Cassa per il Mezzogiorno, politica e lotte sociali nell'Agro Pontino (1944-1961).* Temi di Storia. Franco Angeli.

Marzotto, A and G Schachter. 1983. "Allocation of investments and electoral behaviour in the Italian South." *Quaderni dell?Osservatorio Elettorale* 10:66–88.

McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of econometrics* 142 (2):698–714.

Merito, Monica, Silvia Giannangeli, and Andrea Bonaccorsi. 2010. "Do incentives to industrial R&D enhance research productivity and firm growth? Evidence from the Italian case." *International Journal of Technology Management* 49 (1-3):25–48.

Ministero dell'Interno - Divisione Servizi Elettorali - Direzione Generale dell'Amministrazione Civile. 1954c. *I Risultati delle Elezioni dal 1946 al 1952 - Elezioni comunali dal 1951 in Poi*, vol. Decimo, parte seconda: Italia Centro-Meridionale - Sicilia - Sardegna. Istituto Poligrafico dello Stato.

———. 1954a. *I Risultati delle Elezioni dal 1946 al 1952 - nei Comuni con oltre 10.000 abitanti*, vol. Terzo, parte prima: Piemonte - Umbria. Istituto Poligrafico dello Stato.

———. 1954b. *I Risultati delle Elezioni dal 1946 al 1952 - nei Comuni con oltre 10.000 abitanti*, vol. Terzo, parte seconda : Lazio - Sardegna. Istituto Poligrafico dello Stato.

Moser, Petra. 2005. "How do patent laws influence innovation? Evidence from nineteenth-century world's fairs." *American economic review* 95 (4):1214–1236.

———. 2012. "Innovation without patents: Evidence from World's Fairs." *The Journal of Law and Economics* 55 (1):43–74.

———. 2013. "Patents and innovation: evidence from economic history." *Journal of Economic Perspectives* 27 (1):23–44.

Muraközy, Balázs and Álmos Telegdy. 2016. "Political incentives and state subsidy allocation: Evidence from Hungarian municipalities." *European Economic Review* 89:324–344.

Nagaoka, Sadao, Kazuyuki Motohashi, and Akira Goto. 2010. "Patent statistics as an innovation indicator." In *Handbook of the Economics of Innovation*, vol. 2. Elsevier, 1083–1127.

Nelson, Richard R. 1959. "The simple economics of basic scientific research." *Journal of political economy* 67 (3):297–306.

Neumark, David and Helen Simpson. 2015. "Chapter 18 - Place-Based Policies." In *Handbook of Regional and Urban Economics*, *Handbook of Regional and Urban Economics*, vol. 5, edited by Gilles Duranton, J. Vernon Henderson, and William C. Strange. Elsevier, 1197 – 1287.

OECD. 2016. "OECD Fiscal Decentralization Database."

———. 2017. *OECD Science, Technology and Industry Scoreboard 2017*.

OpenStreetMap contributors. 2017. "Planet dump retrieved from https://planet.osm.org ."

Pack, Howard and Kamal Saggi. 2006. *The case for industrial policy: a critical survey*. The World Bank.

Papagni, Erasmo, Amedeo Lepore, Emanuele Felice, Anna Laura Baraldi, and Maria Rosaria Alfano. 2018. "Public Investment and Growth Accelerations: The Case of Southern Italy, 1951-1995." Eeri research paper series, Economics and Econometrics Research Institute (EERI), Brussels.

Pellè, Loredana. 2009. *Mezzogiorno e Piano Marshall - La Ricostruzione dal 1947 al 1952*. Biblioteca di storia contemporanea. Piero Lacaita Editore.

Pescatore, Gabriele. 1961. *Dieci anni di esperienze della Cassa per il Mezzogiorno*. Rome.

Pettersson-Lidbom, Per. 2008. "Do parties matter for economic outcomes? A regression-discontinuity approach." *Journal of the European Economic Association* 6 (5):1037–1056.

Podbielski, Gisele. 1978. *Venticinque anni di intervento straordinario nel Mezzogiorno.* Giuffrè - Svimez.

Possanzini, Davide. 2000. "L'elaborazione della cosiddetta legge truffa e le elezioni del 1953." *Quaderni dell'Osservatorio Elettorale* (46):49–93.

Qian, Nancy. 2015. "Making Progress on Foreign Aid." *Annual Review of Economics* 7 (1):277–308.

Schochet, Peter Z. 2009. "Statistical power for regression discontinuity designs in education evaluations." *Journal of Educational and Behavioral Statistics* 34 (2):238–266.

Svimez. 2011. *150 anni di statistiche italiane: Nord e Sud 1861-2011.* Il Mulino.

———. 2015. *La dinamica economica del Mezzogiorno - Dal secondo dopoguerra alla conclusione dellíntervento straordinario.* il Mulino.

Toniolo, Gianni. 2013. "An overview of Italy's economic growth." In *The Oxford Handbook of the Italian economy since unification.*

Trigilia, Carlo. 1992. *Sviluppo senza autonomia: effetti perversi delle politiche nel Mezzogiorno*, vol. 54. Il mulino.

Urban, Joan Barth. 1986. *Moscow and the Italian Communist Party: From Togliatti to Berlinguer.* IB Tauris.

Vecchi, Giovanni. 2011. *In ricchezza e in povertà: il benessere degli italiani dall'Unità a oggi.* Il mulino.

Veiga, Linda Gonçalves and Maria Manuel Pinho. 2007. "The political economy of intergovernmental grants: Evidence from a maturing democracy." *Public Choice* 133 (3):457–477.

Ward, Hugh and Peter John. 1999. "Targeting benefits for electoral gain: Constituency marginality and the distribution of grants to English local authorities." *Political Studies* 47 (1):32–52.

WB. 1951. "Italy - An appraisal of the Southern Italy Development Project." Loan series L 133, World Bank, Washington DC.

Wright, Gavin. 1974. "The Political Economy of New Deal Spending: An Econometric Analysis." *The Review of Economics and Statistics* 56 (1):30–38.

Zúñiga-Vicente, José Ángel, César Alonso-Borrego, Francisco J Forcadell, and José I Galán. 2014. "Assessing the effect of public subsidies on firm R&D investment: a survey." *Journal of Economic Surveys* 28 (1):36–67.